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PREFACE

In evaluating Volume 7 of the *Annual Review of Psychology* a journal critic recently observed that he was unacquainted with the procedures whereby the *Annual Review* authors are chosen. Since his lack of knowledge is undoubtedly shared by many other psychologists it would seem proper to indicate here the steps by which those responsible for the writing of this journal's chapters are selected. There are three sources from which names are obtained. (a) After a volume has gone to press the chapter writers are asked not only for suggestions on ways to improve the *Review* but also for the names of at least three or four persons who might be expected to do scholarly jobs of reviewing the many publications of their area for the volume to be published two or three years later. (b) Readers of the *Annual Review* occasionally—but all too rarely—mention persons who, in their opinion, are especially well qualified for chapter writing. (c) The members of the Editorial Committee and the Associate Editor send the Editor each summer their author choices arranged in rank order for each of the topics of the volume in question. The names from these three sources are assembled in lists and are taken to the Editorial Committee's annual meeting where they are carefully considered. Usually three names are decided upon for each chapter, and rarely are acceptances not received from the first or second choices. This surprisingly high acceptance rate suggests that psychologists are extremely cooperative when they are faced with something they feel will benefit their profession. They accept the invitations while knowing that their task will not be an easy one and that virtually their sole reward will be the realization of the worth of their product.

The Editorial Committee which was responsible for the authors of the current volume, the Committee of two years ago, was composed of J. E. Anderson, N. D. Cameron, C. T. Morgan, R. L. Thorndike, and J. G. Darley. The Editor was the late C. P. Stone and the Associate Editor, Quinn McNemar. The members of the Editorial Committee now serve staggered terms of five years' duration. On January 1, 1956, J. E. Anderson's term was completed and he was replaced by D. B. Harris. A few months later N. D. Cameron resigned because of the pressure of other duties. The remainder of his term will be filled by J. McV. Hunt. Mrs. Lillian Rutherford served throughout the year as Editorial Assistant but resigned in November of 1956. The Committee has deeply appreciated her services and regrets her decision to leave this work. She was replaced by Mrs. Adele Fumino. Miss Robbie Bass has again compiled the subject index.

This volume's arrangement has been changed but little from that of Volume 7. A chapter on somesthesia replaces one on taste and smell, and the topic of special disabilities last year's chapter on gerontology. The topic of individual differences has reappeared, and a two-year review of audition fills last year's gap in this area. While the bibliographical style remains unchanged in this volume, the serious reader will be pleased to learn that,

starting with Volume 9, article as well as book titles will be included. In the Preface of Volume 7 it was noted that the attempt initiated by the former Editor to secure a manuscript on "Current Psychology in the USSR" by a Russian national was being continued. However, as success has not been achieved, the Editorial Committee is now considering the selection of some American or Western European who has the requisite knowledge and language facility to do the task. It is possible that a chapter on current Russian psychology will appear in Volume 9.

During the coming year the Editorial Committee expects to reconsider the philosophy which so far has governed the manner in which the *Annual Review* has split the area of psychology into eighteen or so subareas. While over the years a few new topics have appeared and a small number of old ones have been fused, redefined, or renamed, the over-all content pattern has changed very little. It is the Committee's feeling that perhaps some fairly major alterations in the design of the chapters are now needed. Hence, the Committee and the Editor will welcome suggestions for change from the readers of the *Annual Review*.

J.M.B.	Q.McN.
D.B.H.	C.T.M.
J.McV.H.	R.L.T.
P.R.F.	

ERRATUM

Volume 7

page 351, line 42: *for health read* attitude toward health

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VOLUME 9 (1958)

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VISION^{1,2}

BY R. W. PICKFORD

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This review was based on a consideration of about 350 to 400 publications on vision and was concluded in April, 1956. The writer is greatly indebted to the many authors who sent him reprints. In order to keep within the limits of space available a considerable number of researches have had to be omitted, and for the same reason some material has been dealt with very briefly, but it has been possible to deal more fully with other material. Vision is a vast and varied field of research, and the year 1955 to 1956 has been particularly rich in the production of publications of interest. Probably four or five times the space available would be required to do even a semblance of justice to all of them, and to relate them in detail to the work of earlier periods would need a separate chapter and cannot be regarded as one of the present writer's functions within the space allotted. The bulk of the papers omitted deal with human engineering, display and control, instrumental reading and design, and purely physiological and ophthalmological problems, but suitable niches have been found for as many as possible of these at appropriate points in the text. Little or no space has been available for affective qualities in vision or for vision in relation to animal behavior.

VISUAL ACUITY

Tests and techniques.—A lighting unit equipped with two fluorescent light sources, suitable with 22.4" × 23.6" visual acuity charts, is recommended by Odland & Sloan (16), who also suggest an alternative system which is satisfactory. With the Landolt ring Tiffin & Rabideau (21) studied visual acuity thresholds by the constant method, using Muller-Urbach values and 5, 10, 20, 30, 40, and 50 presentations per stimulus. They found retest reliabilities as high as .92 for as few as 10 trials per stimulus and conclude that classical psychophysics has probably tended to use more judgments than necessary. Blackwell's work (3) upon the influence of procedures on the psychophysical measurement of two sensory functions is relevant, together with his studies of the form of visual threshold data (4), followed by the work of Blackwell *et al.* (6) on an automatic apparatus for stimulus presentation and recording in visual threshold measurements.

Foxell & Stevens (8) measured visual acuity with a Landolt ring on a

¹ The following abbreviations are used in this chapter: cff (critical flicker frequency); C.I.E. (Commission Internationale d'Eclairage).

² The writer would like to thank Professor W. J. B. Riddell (Ophthalmology), Professor R. C. Garry (Physiology) and Professor C. M. Yonge (Zoology), who kindly allowed him the use of their Departmental Libraries in the University of Glasgow for his work on this chapter.

white ground, up to 10,000 ft. lux, with surrounds of various sizes. Differences in visual acuity with different types of test targets were studied by Rabideau (20), while Morris, Katz & Bowen (14) studied the checker-board type of visual acuity target and Morris, McGuire & van Cott dealt with the accuracy of the MacBeth Illuminometer in terms of operator variability, calibration, and sensitivity (15).

Dynamic visual acuity.—A series of three important reports have been presented by Ludvigh & Miller (10, 11, 12) on the problems of dynamic visual acuity. The first report dealt with the effects of training upon the dynamic visual acuity of 200 naval aviation cadets and showed that the rate of improvement varied greatly among individuals. A statistically significant difference existed at the first, and at all subsequent trials, between the 20 subjects having the highest and the 20 having the lowest thresholds at the start. The second report, based on a further study of these cadets, showed that the thresholds determined at 20°/sec. and 110°/sec., and the a and b parameters of the equation

$$y = a + bx^2,$$

relating visual acuity to angular velocity of test object were all markedly nonnormal in distribution. Possible causes for this lack of normality were discussed. The third report showed by further analysis of the visual acuity data for the same 200 cadets, who were given a test of 45 min. duration, that a shortened test of 12 min. duration would be adequate for future work.

Other researches.—The effect of differential adaptation of the eyes on threshold sensitivity was studied by Mitchell & Liaudansky (13), and Verplanck *et al.* presented a final report on the response mechanisms at the visual threshold (23). Ten Doesschate (7) reported on the results of the examination of eyes with normal visual acuity in 1,000 Dutch applicants for the post of aircraft pilot. An interesting paper by Pirenne (17) examined the absolute sensitivity of the eye and the variation of visual acuity with intensity of stimulation, while Pirenne & Marriott dealt with the absolute threshold and frequency-of-seeing curves (19). A further paper by Pirenne deals with physiological mechanisms of vision and the quantum nature of light (18). Blackwell's paper on the neural quantum theory in vision should be mentioned (5). It is not without interest that a paper by Altevogt suggests that the visual acuity of a female elephant eight years of age was comparable with that of the horse (1). Mention should be made of Thomson's study of the stimulation of the retina with light fields of small size (22) and of Biersdorf's (2) on the critical duration of visual brightness discrimination for three centrally fixated areas. The results indicate that the transition between the conditions, $It = \text{constant}$, and $I = \text{constant}$, for central fixation, was fairly sharp irrespective of the field size or luminance level, both for absolute and differential thresholds.

Hillmann *et al.* studied brightness thresholds for targets of differing contrasts at eight positions on the temporal retina. A marked drop in threshold

for all retinal positions was found as contrast increased, and there was an optimum point for off-center viewing at each contrast level (9).

FLICKER

Measurements under various conditions.—Differential thresholds for the discrimination of flicker rates between a standard surround and a variable spot of 1° visual angle, were measured for two subjects by Mowbray & Gebhard (35), using the method of adjustment with ascending and descending matches alternately. They followed this paper by a report with Byham on further research with the same apparatus, in which they measured differential thresholds for flicker in the range of one to 45 c.p.s. (29). The standard deviation varied from 0.02 to 0.41 c.p.s., and the relative difference limens ranged from 0.005 to 0.024 c.p.s. There were 375 just noticeable differences of flicker frequency in the range of one to 45 c.p.s.

The critical flicker frequency (cff) relationship to log area was studied by Kugelmass & Landis (33), using a light to dark ratio of 0.05, between 14 and 48 c.p.s. Three men, 25, 26, and 56 years of age, acted as subjects, and 36 different sized patches were used, ranging from 1.27° to 14.60° of visual angle. It was found that the cff to log area ratio was approximately a straight line between 1.27° and 5°, and a negatively accelerating function beyond 6° of visual angle.

The relationship of cff with age and intelligence was dealt with by Colgan (25) for two groups of men, 18 between 65 and 80, and 22 between 81 and 90 years of age. There was a significant correlation with Wechsler-Bellevue Intelligence Test results independently of age. The effect of varying the critical fusion thresholds was studied by Landis *et al.* (34) when nitroglycerine or the cold pressor test was employed. Among other interesting researches, the work of Shikano & Kuwahara (38) suggests that the constants of the Ferry-Porter Law ($\text{cff} = K \times \log I + K'$) are variable with color.

Goldstone showed that a group of high anxiety patients had reduced sensitivity to flicker, greater individual variability, and greater decline in sensitivity to flicker with continued exposure than normal persons or low anxiety patients (30). Davis' experiment (26) suggests that auditory rather than visual flicker fusion rate may be used as a measure of fatigue. An interesting experiment by Goldzband & Clark (31), in which the cff was measured for rats, was inconclusive about the effects of frontal lobe lesions in lowering cff in these animals.

Electrophysiological concomitants of flicker.—Zetterström (40) recorded flicker electroretinograms in 35 infants of the following age groups: 0 to 1 week, 3 to 4 weeks, and 7 to 8 weeks. Within 24 hr. of birth no flicker can be recorded, or only at very high intensities of stimulation. Rutschmann (37) gave an important survey of the history of perceptual cff work and a critical analysis of electroencephalogram and electroretinogram studies dealing with flicker. He favors micro-electrode methods. Watanabe's work on the electroencephalogram induced by flicker deserves mention (39).

Apparatus for flicker research.—Two reports dealt with flicker apparatus. Berger *et al.* described an electronic apparatus which delivers light flashes with intensity, light to dark ratio, and frequency as independent variables (24). A new apparatus consisting of a combination of a small projector and an episcotister was employed by Reuning (36). Results showed that cff variations between individuals were greater than within individuals, suggesting a personality characteristic. There was a retest reliability of .70 and a correlation of .345 with alpha rhythm frequencies.

The stabilized retinal image.—Among the most fascinating researches published on vision in 1955 are those of Ditchburn (27) and Ditchburn & Fender (28) dealing with the stabilized retinal image, the problems of which are not unrelated to flicker. A very ingenious apparatus and technique were used to counterbalance involuntary eye movements, and it is obvious that, since minute points can be steadily fixated in this way, very valuable investigations will arise from the work. Unfortunately it cannot be dealt with more fully here, except to say that the retinal image immediately vanishes when stabilized and that normal vision can be restored by interruptions of the incident light beam at about the rate of normal flicker fusion. Krauskopf & Kalla's work on the effects of retinal image motion on contrast thresholds indicates that the disappearance of stabilized images is the result of the removal of the low-frequency components of normal retinal image motion (32).

ADAPTATION

Effects of various lights on adaptation.—The purpose of the research of Smith *et al.* (67) was to determine the effects upon dark adaptation of exposure to various "red" lights. Differences were found for adapting wavelengths of 601, 626, 640, 675, and 690 m μ . Dark adaptation was most rapid after exposure to light of 626 m μ wavelength. This is interesting to compare with Miles's work (58) in which it was shown that although red light is good for promoting dark adaptation, it does not yield such low thresholds as those gained in the same time after adaptation to darkness. Polinsky & Young (65) tested 10 males by making them look through a red, a green, and a blue filter during dark adaptation for 2, 3.5, 5, and 10 min. A trend analysis of the dark adaptation curves showed that blue is least effective and red most effective in promoting dark adaptation. Green is intermediate but as good as red at the lowest luminance levels. Foveal light adaptation was investigated by Bush (45) as a function of wavelength, using three nearly monochromatic lights, red, green, and blue, under various conditions. The larger adaptive effects occurred for homochromatic and the lesser for heterochromatic combinations of adapting stimulus with test flash. Kinney (56) found that there is a range of intensities over which relative spectral sensitivity does not change from scotopic luminosity function and that the first change in spectral sensitivity with increased intensity is in the long wave half of the spectrum. According to Nomura (62), in a dimly lit room there is a change in the func-

tion of light receptors between 0.005 and 0.1 lux illumination. There is a rapid decrease in light sensitivity when it is over 0.1 lux. It is convenient to mention here Stiles's work on visual properties studied by subjective measurements in the color adapted eye (68).

Measurements of dark adaptation.—Mote & Adair (61) measured dark adaptation after continuous and intermittent previous light exposure and found that it was considerably slowed after intermittent light. Johannsen *et al.* (55) found that dark adaptation increased as the product of the pre-exposure time and brightness rose above a critical value. Differences in the dark adaptation curves obtained for the two eyes tested with a 2° test square, separately and alternately under various conditions, are discussed by Wolf & Zigler (70). Pirenne *et al.* (64) describe experiments to measure the luminance levels required to resolve black Landolt rings of different sizes, and they correlated the results with absolute thresholds, while Hartline & McDonald (64) discuss a method of carrying out simple tests of visual efficiency at low illuminations. Comparisons between light and dark adaptation are given by Baker (43), and Ogilvie *et al.* showed that scotopic acuity was dependent on absolute scotopic threshold (63). Miles (59) compared the use of light sensitivity tests and form perception tests of scotopic vision for 3° test targets. Variations of the scotopic threshold for a point source of light are dealt with by Kruger & Boname (57), who found a consistent relationship between threshold and distance of point stimulus from the center of a large illuminated area.

Physiological problems.—The parts taken by different types of receptors in light and dark adaptation were dealt with by Auerbach & Wald (42). The photochemical and nervous components of visual adaptation are discussed by ten Doesschate & Bouman (49). Mitari & Yagasaki (60) reported on the electric potential in the isolated retina of the eyes of the carp, with light and dark adaptation. The relation between duration of stimulus and visual sensation was studied for two human subjects in dark adaptation by Seo *et al.* (66). Conditions which modify the spectral sensitivity of the human photopic retinal action potential are dealt with by Armington (41). When responses were small their relative spectral sensitivity was nearly independent of the stimulus diameter. According to Crampton & Armington (48) the photopic response gave a curvilinear reciprocal area-intensity relation, while the scotopic response gave a linear reciprocal relationship. Modifications of the human electroretinogram by light and dark adaptation are given by François *et al.* (50).

Other investigations.—Chinaglia & Fregnan (47) showed that although differences in dark adaptation were present in 22 out of 40 anisometropic subjects, these differences were not attributable to the anisometropia itself. Measurement of the refractive state of the eye was made by Chin & Horn (46) under various illumination intensities, and among about half of their subjects there was an increase of refraction as illumination was reduced. Granger (54) tested 20 normal and 20 neurotic subjects in one research and

40 normal and 100 neurotic subjects in another and found a consistent tendency for the neurotics to have higher thresholds over the "rod" period of the adaptation curve and longer perception times than normals. Dark adaptation curves for the pigeon were established by Blough (44) by training the bird to peck one key when the stimulus patch of light was visible and another when it was invisible.

A number of important researches have been contributed by Fry & Ward (51, 52, 53, 69) upon the effects of chromatic adaptation on normal color vision. A monochromator presented adaptive stimuli to the left eye, while a modified Wright colorimeter presented monochromatic stimuli to the same eye and at the same time permitted measurement of chromaticness and brightness changes by means of a tristimulus match with monochromatic components presented to the right eye. The effect of adaptation to monochromatic stimuli of 435, 490, 524, 573, and 741 $m\mu$ on the brightness and chromaticness of various test stimuli was investigated, and special attention was paid to the Bezold-Brücke phenomenon. It was found, under the experimental conditions applied, that the area of the color mixture triangle bounded by lines between 436, 470, and 680 $m\mu$ is dichromatic. It was proved that the polygon of fundamental colors has at least four sides, and the implications of this for the theory of color vision are discussed, together with other important findings.

COLOR VISION

The C.I.E. and other systems.—One of the most valuable pieces of work published during the period under review was that of Stiles (116, 117, 118) on the new determination of the C.I.E. color matching functions. The three primary stimuli were used on 10 subjects for 2° and 10° fields of view. The main problems were as follows: (a) the distance apart of two points corresponding to stimuli equally bright but just perceptibly different in hue in different color regions; (b) the color co-ordinates for surface colors; (c) new studies of the standard visibility function at the blue end of the spectrum, and the possible use of a standard "blue" mixture light of about 445 $m\mu$ instead of 460 $m\mu$; (d) the whole question of possible individual variations in the additive law of color matching.

Rautian's ellipses representing just perceptible differences of color for 36 basic hues have been adjusted by MacAdam (100) to the C.I.E. chromaticity diagram to facilitate their comparison with previous results. Schelling (111) has dealt with the advanced geometry of color. The official German standard color chart (DIN-Farbenkante) is intended as a technical device for practical specification of color, and it has been explained by Richter (108), while Davidson & Hemmendinger (81) have calibrated a system of points which permits rapid and accurate reproduction of colors of any specification lying within the gamut available. In addition, Davidson & Hanlon (80) have described a series of charts derived from the MacAdam ellipses which may be used to simplify and speed up calculations of color

difference. Penndorf (105) has taken the spectral reflectivities of natural objects from Krinov's measurements and computed color parameters for them.

Brown *et al.* studied a sample of 650 trichromatic matches made by one observer in a binocular colorimeter. They regard the normality of the distributions of the three variables, red, green, and blue, as evidence of the trivariate normality of the distribution of color matches for the color space considered (74). Dartnall's study of the interpretation of spectral sensitivity curves should be mentioned (78).

Theory of color vision.—Piéron has pointed out the importance of a study of the parts played by the optical fibers in the reorganization and differentiation of the fundamental messages in color vision (106). Riggs concludes that the exact mechanism of color vision is still largely a matter of speculation but points out that knowledge of the color differentiation responses of the retina is rapidly increasing (109). The ways in which colorimetry can contribute to the understanding of the mechanisms underlying the seeing of colors are pointed out by Judd (95). Houstoun (88) has explained how the three variables of the Young-Helmholtz theory may be replaced by I , s , and σ , the area, position, and breadth of a probability distribution. Segal's theory of color vision has been reviewed by Zigler (112, 123).

Apart from these generalized discussions a series of important papers dealing with quantitative aspects of an opponents theory has been published by Jameson & Hurvich (90, 91, 94), the first being devoted to chromatic responses, the second to saturation and hue, and the third to a theory founded on the Hering system, involving six basic qualities, red, yellow, green, blue, black, and white, arranged in three pairs of opposites. There are, however, on this system only three photochemical substances: red involving the excitation of the first and third, yellow of the first and second, and green and blue of the second and third alone, while white involves all three and black is the result of nonexcitation. The experiments on trichromatic matching do not prove the trichromatic theory but only show that under certain conditions as few as three primary stimuli would suffice. Under other conditions two would be enough [Fry (51)]. Since there are many phenomena which call for an opponents theory of the Hering type, it would seem that Jameson & Hurvich's work is a step in the right direction, and it should be considered in relation to the observations of Fry (51), Boynton (73), and Pickford (144) which support a four color theory.

Fiks (84) carried out a research on the number of psychologically primary hues and receptor types by factorial analysis of intercorrelations between comparisons of 20 Munsell hues, but after the extraction of seven factors, he considered that a meaningful structure had not been demonstrated. The first factor seemed to bring out the contrast of red and green, and the second that of yellow-green and violet. Gregory considers the stability of the Rayleigh equation after adaptation to red and to green lights in a group of 30 normal and four anomalous subjects to be evidence against

a foveal system for yellow (87), but equivalent experiments can be carried out with any color equations, whether they include a so-called primary or not. Boynton (73) has shown that with monochromatic foveal test flashes 0.05 sec. after the onset of colored adapting stimuli, the maxima in the spectral sensitivity curve of the cones in the dark adapted eye can be greatly enhanced, and he has suggested that the resulting over-all sensitivity curves obtained for absolute threshold measurements for red, yellow, green, and blue adaptations are the sum of four underlying curves having maxima at about 430, 530, 570, and 610 $m\mu$, corresponding to a four components theory of color vision.

Rushton has developed an improved technique for measuring the visual pigments in the human eye by physical analysis of the light emerging in an ophthalmoscopic arrangement. For a subject with normal color vision the total pigment corresponded to the normal photopic visibility curve, but the fovea has two distinct pigments separable by bleaching with strong red or blue light. For a protanope there was one detectable pigment whose absorption coincided with his foveal threshold visibility curve. One extremely protanomalous subject was similar to the protanope. In one deuteranope and one extreme deuteranomalous subject there was too little pigment to analyze. The protanope had more pigment than the normal subject. Although this work has so far been done on very few subjects, it promises to be important (110).

Various researches.—Kura0 (97) has shown that the blind spot increases in size when colored test objects are used on a black background, in the order white, yellow, blue, red, green, but when on a white background in the order black, red, blue, green, yellow. The nature of white and the circumstances under which it is seen have been examined by Kellershorn (96) and by Willmer (122), and Weale has dealt with color vision in the peripheral retina (119). An interesting research by Costaridès suggests that x-rays give a faint blue luminosity in the dark adapted eye (77). Problems of color vision in infants have been studied by Malrien (102).

Stevens has given an account of the history of the decibel system of measurement and shows how it can be applied to light with advantage as well as to sound (115). A test of immediate hue memory by Burnham & Clark (75) employs 20 caps of the Farnsworth-Munsell series, together with two caps for practice and 43 for comparison, all mounted in two concentric circles. The test was standardized on 130 technical and clerical workers who had normal color vision and is sufficiently reliable to be of practical value as an aptitude test.

Interesting researches have been carried out by Baker & Mackintosh (72) on the influence of past associations on color judgments, and by Newhall (103) on the effects of width and area of test fields on discrimination of two colors, orange and magenta, while Fender (83) has shown that a shift in the fixation of the eye occurs with a change in the color of the object fixated. Ladd & Pinney carried out a research on empirical relationships with the

Munsell color scale (98), and it is interesting to see Evans & Brewer's studies of observer requirements in color television (82).

Affective qualities of colors.—Pleasant-unpleasant judgments for 316 color samples classified according to the Munsell notation were made by 20 women in an experiment by Stamm (113). The constant terms of the Fourier equations increased in a linear way with chroma and tint. The first two harmonic components accounted for 75 per cent of the fluctuations of the curves of affective value and were related to tint and chroma. Granger (85) reports an experiment using seven sets of Munsell color combinations and an aesthetic measure proposed by Moon and Spencer. Ranking methods were used. Moon and Spencer's measure seemed of little use for prediction, and in another paper Granger (86) puts forward an empirical formula for the prediction of preferences for binary combinations of hues. McElroy showed that there is a positive relationship between certain types of orderliness and of dislike for special shades of green and brown which might have infantile anal or urethral significance (101).

Animal color vision.—The spectral sensitivity curve for the aquatic flatworm (*Dendrocoelum lacteum*) was found by Pirenne & Marriott to be similar to that of the human eye without a lens (107) but does not imply color vision. Ilse (92) and Ilse & Shak (93) found that three species of Indian bees have color vision similar to that of the European honey bee, but all are more or less red blind, like the human protanope. Work on the median ocellar nerve of the locust by Hoyle (89) shows that the ocellus must act mainly to indicate light intensity changes. Other work on the ocelli of the locust and of *Calliphora* has been carried out by Cornwell (76). Two researches have been devoted to the eyeless hag (*Myxine glutinosa*), one by Newth & Ross (104) and the other by Steven (114). The eyeless hag responds to light by active locomotion, photoreceptors being located in the skin, and its reaction time to light is inversely proportional to the intensity of the stimulus at less than 10 effective foot candles. Above this level it has a constant value. The hag is most sensitive to light between 500 $m\mu$ and 520 $m\mu$, and the response is proportional to the penetration of light through sea water but does not imply that the hag has color vision.

Three photosensitive pigments were found by Dartnall (79) in the dark adapted retina of the bleak (*Alburnus lucidus*) with absorption maxima at 510 $m\mu$, 533 $m\mu$, and 550 $m\mu$. The 510 $m\mu$ pigment was shown to be distinct from the frog's visual purple of 502 $m\mu$ absorption maximum. Weale reported bleaching experiments on 27 albino and 10 pigmented anaesthetized and curarized guinea pigs. Bleaching light sources were red, yellow and green, or blue, and it seems that four photosensitive components may be present (120). Bleaching spectra were also obtained by Weale (121) from dark adapted grey squirrels, anaesthetized and curarized. These animals have a pure cone retina. The mean spectrum showed maximum density change at 535 $m\mu$. Electroretinograms were taken from the grey squirrel under light anaesthesia in dark and light adaptation by Arden & Tansley

(71), and the sensitivity curve showed its maximum at about 530 $m\mu$. There was no Purkinje effect. Lennox & Madsen report that the retinal potential in response to one color in the anesthetized cat may be reproduced in response to any other color by suitably adjusting the intensity (99), which accords with the theory that the cat is totally color blind.

COLOR BLINDNESS

General.—Farnsworth points out that complete specifications of stimuli in terms of physical standards and psychological variables affecting vision, and better definition of experimental populations are needed for the study of normal and defective color vision (129). Cavanagh reviewed the problems of the use of the Ishihara test for color blindness (125), and Pickford considered weak and anomalous color vision in industry, pointing out the need for adequate tests and recommending a four-color anomaloscope test (146). Wright's interesting paper on defective color vision was relevant to these problems (154). Graham *et al.* have studied luminosity functions for five protanopes, five deuteranopes, one woman deuteranopic in the right eye only, and seven normal subjects. They observed the expected lowering in luminosity for protanopes in the red part of the spectrum, and found that deuteranopes showed a reduced sensitivity between 575 $m\mu$ and 415 $m\mu$, which was also present in the deuteranopic eye of the unilateral deuteranope (130).

Anomaloscope tests.—In an important paper on the use of the Nagel anomaloscope, Schmidt (150) deals with adaptation, the elimination of training effects, the number of readings required, the application of Trendelenburg's anomalous quotient and its frequency distributions, the determination of red-green matching ranges, retest reliability, and other problems. The results of testing 864 unselected men of 17 to 23 years of age are given in detail. The following frequencies of red-green major defectives were found: 1.13 per cent protanomalous, 3.96 per cent deuteranomalous, 0.57 per cent protanopes, and 1.25 per cent deuteranopes; 0.23 per cent color amblyopes and 0.23 per cent color asthenopes; giving 7.37 per cent in all. These proportions are interesting to compare with other results (144, 145, 147). Hioki & Nakamura (132) and Crone (127) have described modified forms of anomaloscope. Willis & Farnsworth's important comparative study of six anomaloscopes should be consulted (153a). The writer considers that in all anomaloscope tests it would be far better to work with plain scale readings than with the logarithms of ratios recommended by Schmidt.

Pseudo-isochromatic and other tests.—Kettesy (136) has studied seven pseudo-isochromatic tests and considers that between 1 per cent and 5 per cent of subjects are doubtfully defective on them, while he regards these tests in general as ineffective for discriminating types and degrees of defect. In Paris 3,873 men and 3,735 women were tested by Kherumian *et al.* (137) with seven pseudo-isochromatic tests, and he found 9.33 per cent of red-green defective men, divided into 2.57 per cent of protans and 6.76 per cent of

deutans. He estimated the percentage of defective women, assuming the two-locus theory of Waaler, at 0.52 per cent, and found an actual frequency of 0.51 per cent, which is a remarkable agreement with expectation. He confirms the two-locus theory and Pickford's finding that heterozygous women usually show small defects as a result of the incomplete dominance of the normal genes (144).

Hardy *et al.* have given a description of the HRR plates applied to 780 subjects not randomly drawn, (a) to separate color vision defectives from normals, (b) to classify types of defects, and (c) to indicate degrees of defect, for which they claim considerable agreement with the Nagel anomaloscope (131). The Dvorine pseudo-isochromatic plates have been studied by Crawford (126) on 65 color normal and 11 defective subjects, while Peters (143) describes results obtained with this test on 800 men in comparison with the American Optical Company's test, and the Dvorine test was reviewed critically by Murray (141). The Eastman Color Temperature Meter was used as a color naming test by Mayer & Zaccaria (140) in comparison with other tests, and they obtained fairly high agreement. The Illuminant-Stable and American Optical Company's tests were compared by De Nittis (142), who found the IS test more difficult but the AO test equally as stable. The Ishihara test was applied by Dunn (128) to 79 European boys, 26 European girls, 118 Negroid boys, 92 Negroid girls, and 21 Mexican and other boys, in two Chicago schools. She found that Negroid boys and girls made significantly fewer errors than European boys, and this difference was more marked when the test was confined to the plates recommended by Pickford as the most reliable (144). A new set of pseudo-isochromatic plates has been introduced by Umazume *et al.* (153). Three pseudo-isochromatic plates for the detection of tritanopic defects were published by Kalmus (135). The position with regard to all pseudo-isochromatic tests remains unaltered. They are capable of acting as cut-out tests for major defectives, as shown by Sloan & Habel (152a), but for finer discrimination they are all very unsatisfactory.

Schmidt has shown the diagnostic value of Maxwell's spot in testing foveal color vision (149), and Isobe has devoted an interesting research to Maxwell's spot (133). The relationship of color vision, age, and visual acuity was studied by Kleemeier (138), and of color vision and age by Janolšková (134), who makes the remarkable claim that no sex difference in frequency of defectives was found before the age of 8 years, a result not in agreement with other findings on children (124a, 144). Nevertheless, he finds, like other workers (125a, 152b), that color sensitivity falls off considerably with old age, partly as a result of pigmentation of the transparent parts of the optical system.

Color blindness and signal lights.—Three interesting papers have been devoted to the problems of signal lights in relation to color blindness. Bally (124) examined 1,000 boys from primary schools in Basle with pseudo-isochromatic and anomaloscope tests and found 9 per cent of defectives, 4.1

per cent deuteranopes and 1.1 per cent of protanopes, leaving 3.8 per cent anomalous. Out of 76 color blind boys, 17 made no mistakes with traffic signals, 45 made mistakes which were not dangerous, and 14 made dangerous mistakes. Sloan & Habel (151) studied empirically Judd's three-color system for signal lights on instrument panels, which are intended to be suitable for defective and normal subjects. They consider that this system would have some success, and they have also given experimental determinations of the capacities of 24 color blind observers to distinguish red and green point stimuli falling within the same three-color panel light system (152).

Inheritance of color blindness.—As a result of a study of 47 individuals affected by tritanopia, Kalmus (135) concludes tentatively that this condition is generally caused by one or more autosomal dominant genes. The inheritance of color vision defects in general has been reviewed by Pickford (147), who also gave statistical data showing that the expectation that there would be more anomalous than dichromatic women in proportion to men is borne out (148), thus supporting the order of dominance theory of Franceschetti. Kherumian *et al.* are devoting intensive research to the inheritance of color blindness (137). A case of total color blindness has been described by Leurent (139).

CONTRAST-EFFECTS AND IMAGERY

Contrast-effects.—The hues of negative after sensations of colored surfaces were determined by Wilson & Brocklebank (167), who found good agreement among a number of subjects and showed that the complementary hue of an after sensation is determined by the hue of the stimulus independently of relative lightness and purity. The locus of any hue continued smoothly through the white point of the C.I.E. chromaticity diagram into the locus of the hue of its after sensation.

By means of a binocular matching method simultaneous brightness induction was studied by Heinemann (159) as a function of test and inducing field luminances, and he dealt with inducing fields of luminance much greater, much less, and almost as great as, or just greater than the test field luminances. It was shown by Diamond (156) that test field apparent brightness decreases as the area of the inducing field increases when the inducing field luminance is equal to or greater than that of the test field. Diamond *et al.* compared the methods of limits, of adjustment, and of constant stimuli in an experiment on simultaneous brightness contrast (157). The results showed no difference in the characteristic shape of the contrast curve for the different methods.

When a colored figure and neutral background are equally luminous, and fixation is maintained at the center, the figure appears to lose clarity and fade into the background. This is known as the Leibmann effect. Orzack & Schlaegel (162) have shown with a new apparatus that in binocular vision the larger a colored diamond figure in proportion to the neutral background the more frequently, and the smaller the diamond the less fre-

quently, the figure disappears. An interesting study is that of Burnham & Jackson, on the verification of Mach rings by numerical differentiation (155).

Figural after-effects.—Five papers were published on aspects of figural after-effects. Summerfield & Miller (164) conclude that differing mechanisms must be postulated as underlying after-effects and illusory displacements, and therefore that neither Köhler and Wallach's nor Osgood and Heyer's theory is capable of immediate extension to cover illusions. In two experiments, each on 10 observers, Hochberg (160) showed that when figures differ from their grounds in chroma and brightness, figural after-effects occur which do not vary with change in chroma. As a result of his experiments he concludes that Köhler and Wallach's physiological model, which is isomorphic to experience, will need revision. Sutherland studied the relation of figural after-effects, retinal size, and apparent size (165). An experiment which suggests that metabolic efficiency is a useful concept as a common determinant of various measures, such as reaction time, basal metabolic rate, thyroid function, circulatory structure, somatotype, and figural after-effect was reported by Wertheimer (166).

Imagery and synaesthesia.—McKellar & Simpson published an interesting study of hypnogogic imagery (161) and an investigation of synaesthesia (163). In the latter it is suggested that the convention should be adopted of naming the subtypes of synaesthesia in such a way that the first of two words should refer to the image experience and the second to the sense experience. "Visual-auditory" would be different from "auditory-visual" synaesthesia. These authors distinguish "synaesthetic description" from synaesthesia in the strict sense. They found that the visual-auditory type of synaesthesia was the most frequent, but give instances of 13 other kinds, including seven induced by mescaline but not reported as occurring spontaneously.

In an interesting research Drever took occipital electroencephalogram records for 37 blind (19 early blind and 18 late blind) and 37 sighted subjects during the performance of two spatial tests (158). The subjects were divided into three familiar alpha-rhythm types, but these types did not differ significantly from one another in terms of scores on the spatial test, while alpha-rhythm was most frequently found among the blind. Drever suggests that these results are a problem for those who claim that absence of alpha-rhythm during mental work is associated with the use of visual imagery.

FORM AND SIZE

General.—Ronchi has given a summary of work on the perception of size, from Euclid and Ptolemy to Alhazen, and from Priestley to Fechner, Hering, Jaensch, Katz, and Thouless (195). In an interesting review of facts, theories, and experiments on perceiving, Vernon (207) has made out a strong case for the use of the concept of the "schema," which originated with Head and Bartlett. Other general studies of perception are attributable to Michotte (190), and to Gibson (180) who points out that the value of hypo-

thetical conceptions like assimilation, expectation, and habit should be tested by experiments on the "effort after meaning." Postman (191, 192) emphasizes the continuity between methods and concepts in learning and perception and reaffirms the importance of the associationist theory of perceptual learning, and Gibson & Gibson have made a reply (181). Razran puts in a plea for two learning levels, conditioning and perceptual learning (193).

Form.—An experiment is reported by Bitterman *et al.*, in which factors affecting the relative difficulty of simultaneous and successive discrimination were studied (172). The results revealed a variety of discriminative processes, distinct yet interrelated. A paper by Towe (206) dealt with figure discrimination and the mediation of equivalence responses.

Deutsch has set forth a theory of form recognition which is intended to account for the abilities to abstract form independently of place, inclination or size, for the equivalence of the shapes of mirror images and squares and circles, in the rat and octopus, and for the survival of these abilities following extensive lesions of the striate area (176). Binder (171) describes a statistical model of the process of visual recognition.

The selective effect of instructions on the accuracy of report for tachistoscopically presented stimuli has been studied experimentally by Lawrence & LaBerge (188), while Fitts *et al.* describe a method for generating classes of figures which can be specified precisely in probability terms, called metric figures, and give some interesting data about their recognition (179). In a tachistoscopic study Smith & Henriksson used the well-known illusion of a line design superimposed on a square. Subjects judged the apparent form of the square. The deformation seemed greatest in the middle of an exposure series and was the result, not of mere repetition, but of the pre-stages leading up to conscious perception of the lines (203). In a research on the "tunnel effect" and apparent transparency Glynn showed that the form of a grey rectangular figure moving across the ground glass screen of a camera to which an opaque screen of lower brightness was attached, appeared in three ways, as an unshaped darkening, as a shaped darkening, or as transparency (183).

The intimate relationship between memory and perception is discussed by Wallach (208). Three experiments on the effect of symmetry on memory for patterns are reported by Attneave (169), the results of which are believed to constitute an important clarification of the Gestalt doctrine that "figural goodness" is favorable to memory. Anderson & Ross studied memory for items in a matrix (168). Drever gave 74 subjects, grouped as early blind, late blind, and sighted, three spatial tests and concluded that none of the differences found was a result of the age of onset of blindness (177). In a study of 69 patients with various cerebral lesions, Battersby, Krieger & Bender found no differences between visual and tactile discrimination learning scores within any of the groups classified according to the areas of lesion (170).

Brown describes an improved method of measuring rate of apparent change in a dynamic ambiguous figure (174). He suggests that the same

physiological process is responsible for the apparent changes in an ambiguous figure and for the figural after-effects. Fisichelli & Rockwell show that electro-shock therapy tends to reduce reversal rate for ambiguous figures in depressed patients (178).

Size.—Solley & Lee carried out an experiment on perceived size (204) to compare the hypothesis of Bruner & Postman (175) with the Gestalt theory of closure, and concluded that the closure theory was not substantiated. The size of a luminous rectangle seen in a mirror at various distances was equalised with the apparent size of a standard at a distance of 1 m. in monocular and in binocular vision by the subjects in Roelofs & Zeeman's experiment (194). From the results the conclusion is drawn that convergence can influence apparent size and distance but that both are also influenced by a number of other factors. Künnapas showed that the apparent length of a line is a logarithmic function of the area and size of a square frame in which it is presented (187). Brown (173) measured the difference between apparent size on the right and left sides of the fixation point monocularly with three subjects over periods of four to six weeks and showed marked progressive changes in all subjects.

The perception and estimation of object size was studied as the distance of a standard varied from 100 to 4000 ft. by Gilinsky (182), out of doors under conditions affording many distance cues. "Objective" instructions gave matches in size which increased with distance, exceeding size-constancy. "Retinal" instructions gave matches in size which decreased with distance.

Stevenson & Bitterman's experiment on the distance effect in the transposition of an intermediate size by children of four to six years of age (205), and Kelvin's account of the discrimination of size by sight and touch (186) cannot be reported here in detail, nor can Hunter's interesting experiment on children's reactions to varying form and size (184).

Experiments with lenses.—An interesting group of experiments with lenses is reported by Ronchi & Ercoles (197) and Ronchi (196). With lenses of 0.5 to 20 diopters, every time observers could estimate the distance of objects they could localize the images, but when judgments of distance were prevented, using lenses of 10 to 71 diopters, the localization of the image varied in a very irregular way and did not follow any known general law. Ronchi & Zoli (199, 200) have carried out various experiments on the apparent sizes of objects seen through a telescope and found an important psychological factor in the results. The degrees of magnification do not follow the same law as that which governs the size of the retinal image. These authors have also taken into account psychological factors, such as spatial localization, in studying the Aubert-Förster effect and the resolving power of lenses and of the eye (198, 201), and they have shown that when objects are viewed through a telescope, if they are unknown objects the law of visual angle constancy holds, but when they are known objects there is an effect intermediate between this and the law of apparent size (202).

Affective qualities in form perception.—In an experiment on Scottish

children McElroy showed a tendency for boys to prefer disguised female configurations (rounded shapes) and for girls to prefer disguised male configurations (long and angular shapes). He discusses the implications of this for psychoanalytic theory (189). Jahoda has confirmed these results in a repetition of the experiment on West African children in the Gold Coast, but found a less marked contrast of sex preferences presumably because of the less amount of repression (185).

DIRECTION, POSITION AND MOVEMENT

Slant, position, and direction.—A reduced retinal shape without stimulation for slant can induce a whole family of apparent shapes, but with a strong tendency for the frontal shape to predominate. An illusory slant can induce an illusory shape, and the constancy of perception of shape appears when stimulation for slant is given. These points are brought out by Beck & Gibson (209), who describe three experiments on the relation of apparent shape and apparent slant to the perception of the shape of objects in space.

Three experiments were made by Heinemann & Marill (213) on the change in apparent tilt of lines after long inspection, and it was concluded that the results represented an alignment effect and not adaptation to the norm in Gibson's sense. In the adjustment of a thin black line to be parallel to another line, Rochlin found that oblique lines yielded greater variance of settings than did horizontal lines (224). In a study of the adjustments of a pivoted rod to the apparent vertical, with head tilted to the right and to the left, Sandström has shown that there are interesting sex differences with tactual-kinaesthetic, but not with visual adjustments (225). The effects of differentially structured visual fields on perception of the vertical were studied by Weiner (229). An interesting group of researches were devoted to centration of regard, localization of a visual point stimulus, and to certain visual illusions, by Piaget (218), Rey & Richelle (223), Rey (222), Piaget & Morf (219), and by Piaget & Pène (220).

Wapner & Werner (228) report two experiments on the effect of asymmetry of a test pattern induced by similarity on ego-centric localization of space. The significance of the experiments is their demonstration that the laws of organization have an effect which can be measured in behavioral terms. A motor theory of visual ego-centric localization has been proposed by Bruell & Albee (210).

The main conclusion of an experiment by Hunter on children's recognition of inverted pictures was that their perception of form was not independent of spatial position (214). Edwards' experiments do not support the theory that colors have in themselves the quality of depth (211). Hunton's experiment on the tactile-kinaesthetic perception of straightness in blind and sighted subjects is of interest here (215).

Movement.—Researches on the perception of movement include that of Reid on blindfold subjects (221). He showed that the extent of movement across the body is underestimated, while movement towards or away from

the body is overestimated, and it is not clear whether vertical-horizontal illusions and illusions of movement simply co-exist or are functionally related. Jones & Bruner described four experiments in which the relationship of stimulus context to general expectation of movement was shown to be an important source of variation (216).

According to Leibowitz, with short exposure durations of 0.125 sec. the velocity threshold is not changed by reference lines, but with a long exposure time of 16 sec. the presence of reference lines lowers it by as much as 48 per cent (217). The recognition of a moving visual object is a function of the distance, size, and velocity of the object, as shown by Hagino (212). Perception of depth movement was shown by Smith (227) to improve with increasing brightness of the test object and with binocular over monocular vision, but the apparent size of the object had no effect. An explanation in terms of exaggerated inhibitory effects, to account for the reported inability of some brain damaged patients to see apparent motion, was supported by experiments carried out by Shapiro (226).

SPACE, DEPTH, AND BINOCULAR VISION

General.—A useful catalogue of translated material on space perception has been compiled by Mann (252), and the bibliography of research reports of the United States Office of Naval Research, on physiological psychology, should be mentioned (231). Piaget has taken up the question of how the child succeeds in elaborating the concept of Euclidean, homogeneous, and isotropic space, and of projective space with its rules of perspective. He concludes that geometrical space is not perceptual but is an operational continuum attributable to the co-ordination and internalization of actions (255). Gibson *et al.* describe the construction of an optical pseudo-tunnel and discuss its use as an apparatus for inducing and controlling the perception of surface and space (239). An instrument for the study of binocular vision was described by Harker (242).

Size, shape, and binocular vision.—Langdon's experiments carry the traditional type of shape-constancy investigations into the realm of three-dimensional solids (249). The results indicate that solids possess perceptual properties not shared by simple surfaces. Subjects react to the stimulus-solid in terms of a conceptual schema or reference frame involving higher mental processes other than those of perception. Babington Smith has considered in the light of Gestalt psychology certain phenomena in binocular vision: the effects of varying convergence and accommodation at different distances; "Hillebrand's Avenue" in which the locus of equal apparent separation of two parallel lines receding into the distance is curved; Thouless's observation that the sides of a box viewed binocularly in perspective seem to diverge under certain conditions because of the suppression of monocularly seen parts of a doubled binocular image (230).

It was shown by Roelofs & Zeeman (259) that size constancy is lost with monocular vision and preserved with binocular vision only up to 4.5

m. Convergence can influence apparent size and distance. Leibowitz & Walker determined the magnitude of binocular brightness summation as a function of field size at two levels of luminance (250). Size was significant but luminance was not. Gilinsky studied the relation of perceived size to perceived distance (240). Hochberg & McAllister (243) examined the connection between relative size and familiar size in the perception of represented depth.

Depth, distance, and binocular vision.—Coules (233) showed that in apparent distance a brighter object farther away is equivalent to a dimmer object nearer, both in binocular and in monocular vision. Katz & Schwartz studied the Pulfrich effect of depth perception, produced by image disparity when one eye is less brightly stimulated than the other, and showed that such an effect is still found when each eye has monocular stimulation in succession (247). They claim that this goes against the latency hypothesis for this phenomenon, based on binocular vision. Westheimer & Tanzman (270) experimented on qualitative depth localization with diplopic images, and showed, among other things, that stimuli with uncrossed disparity were more correctly localized than with crossed disparity. A note on monocular depth perception is given by Pollack (256). Perspectoid distances are discussed by Rasmussen in relation to a series of experiments on certain paradoxes, which show that the phenomena under consideration do not allow of treatment in the usual logical terms but must be approached in the terms of logical positivism (258). Wapner *et al.* (266, 268) have dealt with changes in psychological distance and space localization under conditions of danger.

Two experiments to determine the effect of training on absolute and on relative judgments of the distance of variable targets are described by Gibson *et al.* (236). Experiments to determine the effects of lateral target separation on commonplace depth perception are described by Teichner *et al.* (264, 265). Distances of 100 to 3000 ft. were used, and sophisticated subjects tended to make finer discriminations than unsophisticated subjects, especially at greater distances. Observations on binocular depth perception have been published by Christensen (232).

In an experimental situation based on the Howard-Dolman apparatus, equality of judgments of the relative spatial positioning of two large targets was studied by Teichner *et al.*, when the targets were at different distances and on different types of terrain (263). In a further research with a modification of the Howard-Dolman apparatus, Dusek *et al.* showed that practice tended to increase precision of judgments of depth, and established some other interesting points (234).

A mathematical analysis of motion perspective was made by Gibson *et al.*, who presented it in terms of the optical flow-pattern reflected from a surface to an eye (238). Gibson (237) and Hochberg & Smith (244) have studied the problem of aircraft landing-strip markings and the optical expansion pattern.

A "space eikonometer" and other points.—Gillot (241) has described a

"space eikonometer" for measuring the differences in size of the ocular images in binocular vision. In 42 per cent of an average sample of the population he found differences in size of the retinal images greater than 0.8/100, in 7 per cent of the sample there were differences greater than 3/100, and in 8 per cent he found aniseikonic errors associated with symptoms of eye strain or related to it. He suggests that the eikonometer might be a useful addition to the ophthalmologist's apparatus for testing patients. He also studied the Pulfrich effect in the apparent tilt of the fronto-parallel plane at 3 m. distance, when the illumination of one retinal image was reduced relatively to that of the other, and he studied the longitudinal horopter at a fixation distance of one meter.

Junes points out that there are two kinds of horopter, the objective, passing through the binocular fixation points and represented by a single line, and the subjective horopter, which is the locus of the projection of congruent and incongruent point-images which, found on the cyclopean retina, are not fused. It is exclusively the basis for physiological diplopia (246).

Rady & Ishak (257) find that stereoscopic acuity decreases as the angle of separation of two test targets increases, that convergence has only a secondary effect on stereoscopic vision, and that stereoscopic acuity does not change over a wide range of luminance levels. Lyle & Foley have studied binocular vision with special reference to peripheral fusion (251), and Mori claims that stereoscopic vision is found over the whole binocular field (253). Morrison has carried out some interesting experiments on stereopsis in the presence of diplopia (254). Faulty binocular vision has been studied by Simon (260), who deals with its diagnosis and treatment along physical, physiological, and psychological lines.

Distortions of the visual fields.—Kohler describes experiments in which prolonged distortions of normal optical relationships are made and concludes that stimulus-variables or stimuli of a higher order are able to regulate sensibility in a multiple way (248). The Innsbruck studies of distorted visual fields are thought by Werner & Wapner (269) to advance the understanding of perception in sensory-tonic terms.

The conditions relating to upright vision with inverting spectacles and the stages of learning to see upright while wearing them are explained in a simple way by Erismann (235). Snyder & Pronko (261), who described an experiment in which a subject wore inverting spectacles for 30 days and carried out various practice tasks before, during, and after this period, apparently did not obtain such complete re-instatement of upright vision as Erismann and Kohler.

Binocular vision and form perception of animals.—In a general article on the eyes and vision of animals, Tansley has dealt with comparative aspects of dark adaptation, visual acuity, color vision, perception of movement, and binocular vision (262). Weale has suggested that the forward pointing eyes often found in deep sea fish give binocular vision in order to achieve greater light sensitivity (267). It has been generally assumed that

olfactory stimuli play the most important parts in attracting male insects to the females, but Ilse & Mulherkar find that visual perception of size, shape, and relative brightness are more important in the common housefly (245).

GENERAL

In conclusion several general works deserve mention. Ronchi (275) has pointed out that optics must be considered as the science of vision and not as a chapter of physics, and he asks whether we should not have two terms for "light," one referring to light as seen and the other to the measurable light rays of the physicist (276). Parsons has given a general introduction to the physiology of vision (273) and Tansley a bibliography of British books and periodicals on vision (277). A valuable summary of present-day problems and findings in sensory psychology, including a section on vision, has been given by Ratoosh (274). Bakan (271) has devoted a study to current theories of perception. An interesting problem was studied by Bessière *et al.* who investigated photophobia and its causes and put forward a general neurological theory to explain it (272).

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HEARING^{1,2,3}

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The last two years of research in hearing have produced data in many areas and psychology is but one of these. If this review were confined to this one area alone there would be much to cover but some of the significant advances would be omitted; so the reviewer must pick and choose. The selection of publications for review here is based primarily on whether anything new has been added to our understanding of the way in which the ear works or whether any newly revealed or well established characteristic of auditory phenomena is more accurately described. Even with this selection there is more than can be covered carefully; so certain present day trends in research reporting are followed.

Psychological phenomena in hearing used to be of interest for their own sake and, of course, still are, but the reporting of them has taken on a different emphasis. Many reports are appearing in which the traditional characteristics of hearing are investigated in the abnormal ear, and invariably these are carried out in an attempt to help explain hearing in physiological terms.

This emphasis has had a counterpart in that research into the morphology of the more peripheral structures of the ear is receiving new attention, and more information is being accumulated that shows the influence of physiological processes on the traditional psychological aspects of hearing. It would be impossible to ignore these trends; so the anatomy and physiology of the ear cannot be passed by. This chapter covers, then, (a) anatomy, (b) mechanical properties of the ear, and (c) physiology, followed by (d) intensity discrimination and loudness, (e) fatigue, (f) frequency discrimination and pitch, (g) other subjective attributes, and (h) hearing in communications.

ANATOMY

The reason for precise controls in procedure and statistics in psychological research is that there is the ever present characteristic of individual differences. How much of this is inherent in the morphologic organization and how much is attributable to organic disturbances is a perennial question,

¹ The survey of the literature to which this review pertains was completed in May, 1956 and also covers the period from May, 1954 to May, 1955 which was omitted in Volume 7 (1956).

² The following abbreviation is used in this review: DL, difference limen.

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the answer to which will be a long time coming. But, in audition, as research techniques improve more facts are revealed about the elementary basic anatomical structures responsible for the normal and abnormal hearing process.

Three recent reviews on the neuroanatomy of the ear may serve as a source of material for those interested in neural systems associated with auditory function. Ludin (93) covers the neural supply to the external ear and auditory meatus, tympanic membrane, middle ear, eustachian tube, and some of the sympathetic innervation but does not say anything about the inner ear. Mitchell (108) reviews the autonomic supply to the throat, nose, and ear with good illustrations and references. Galambos (46) in an excellent treatise reviews the anatomy and function of the neural supply to the inner ear and pathways of the auditory nerves.

Internal ear.—Histology, which has always been based on an accumulation of more or less cookbook procedures, is becoming more of an exact science. Through the researches in histochemistry the reasons for certain stains having an affinity for particular tissues are becoming established, and there have been a few attempts to apply these to the study of the ear. Plotz & Perlman (122) used bats caught in caves during hibernation and applied the Hotchkiss method of staining to the frozen and dehydrated tissues and pointed out the tissues that took on the same stained appearance. The reasoning is that tissues which stain alike must be alike or at least engage in the same chemical activity. Vosteen (180) has used a similar approach finding identical staining reactions in the stria vascularis, the sensory epithelium, the ganglion cells of the spiral ganglion, and the band of the spiral ligament lying under the Claudius cells. Churchill, Schuknecht & Doran (23) in a histochemical study of the dissected sensory areas of the cochlea demonstrated acetylcholinesterase in the region of the nerve endings of the hair cells.

A few years ago Engström, at the Caroline Institute in Stockholm, reviewed what the use of the light microscope has revealed about the structures of the inner ear (35). Nothing new was added, but there were some fine photomicrographs. Since then he and his co-workers have turned to the electron microscope, and three reports concerning the hair cells and their supporting structures appeared in 1953 (37, 38, 39). Recently Engström & Sjöstrand (36) have reported upon the structure and innervation of the cochlear hair cells. It is surprising that with all the magnification available through the electron microscope not much more has been revealed than has already been described by the use of the light microscope. But it is nice to have the confirmation that this study brings. There is no doubt now that the nerve fibers do not enter the hair cell but end in clublike structures at the base of the sensory cell. The external spiral fibers lying along the Deiters' cells or in between different rows of Deiters' cells are described, but these have been seen with the light microscope. Some of the details of the hair cells are clearly pictured, for instance the so-called Hensen body near the top of the cell and

the Retzius body between the nucleus and the base. The fact that these structures have the names of the early investigators attests to the accuracy of their observations.

Two studies by phase contrast microscopy have provided more interesting photomicrographs but nothing new [Katsuki & Covell (67) and Vago (178)]. This technique may be promising since it provides a means of working with very fresh, unstained material.

Another technique that may prove profitable in the study of the development and possibly even the function of the sensory cells is the *in vitro* cultivation of the auditory vesicle removed from the embryo. So far chick embryos have been used because of the accessibility. Friedmann (44) repeating a technique of transferring the vesicle every 48 hr. to a new medium similar to the method used by Fell (41) achieved almost complete differentiation of the tissues, with the sensory papilla, such as the bird has, almost completely formed. A more promising technique was explored earlier by Lawrence & Merchant (78), in which the vesicle was not transferred and so could be observed as differentiation took place, making the membranous portion directly accessible for study in the living state.

Blood supply, circulation, and fluids.—Much notable progress has been made in the study of the blood supply to the cochlea. Smith (147) following a previous report on the capillary vessels of the cochlea of the guinea pig has carried her investigations further to the study of the capillary areas of the cat and human. Aside from the intricate system within the stria vascularis she describes an extensive network in the region of the spiral prominence. Large arterial vessels enter this region, and the smaller vessels form a narrow band spiraling around with the prominence. The networks in the stria vascularis, the spiral prominence, and upper and lower portion of the spiral ligament are separated by vascular supply and drainage. This makes possible small segmental variations of circulation so that disturbances may be confined to one area without necessarily spreading to another. This possibility coupled with the observation of Lawrence (76) that the small capillaries of the spiral prominence intertwine among the root-like cells of the external sulcus and are enmeshed in continuation fibers of the basilar membrane points to an understanding of the control of nutrition to the organ of Corti.

More studies of a functional nature are needed, and a start on these has been made by Weille and his co-workers (191, 192, 193) who have observed and photographed, through a small hole made in the bony capsular wall, the flow of the blood in the spiral ligament. These investigators report that the rate of blood flow varies from time to time, and they observed the intermittence of linear flow in arterioles, arteriovenous arcades, capillaries, and venules. These results have been confirmed and produced at will through the use of drugs by Seymour (141) and by Irwin, Weille & Burrage (62). Perlman & Kimura (119) using the same technique have observed the action of these minute vessels in response to stimulation of associated nerve supply.

These will be discussed presently when the physiological activities of the ear are considered.

The fluids of the inner ear are still receiving attention. Ferreri & Casorati (43) have examined endolymph and perilymph of both humans and animals by means of the electron microscope and could detect no species difference in these liquids. They describe and picture the crystalline structure but admit the inadvisability, on the basis of this alone, of drawing conclusions about the chemical and physico-chemical characteristics of the liquid constituents.

Some time ago Lempert *et al.* (80) in a report on the structure and function of the cochlear aqueduct concluded from an investigation on monkeys that, although the cochlear aqueduct is normally patent, there was no evidence of flow of cerebrospinal fluid or perilymph through it. Recently Waltner (182) in a study of the chemical composition of perilymph concluded that there must not be a free flow between subarachnoid and perilymphatic spaces because of the great difference in composition of the two fluids of these areas. Vilstrup, Jensen & Koefoed (179) have reported further on their studies of the labyrinthine fluids from the shark.

The true story of how we hear depends basically upon a thorough knowledge of these anatomical structures and their functional interrelationships. In the meantime many investigations are telling us more about the behavior of the ear when stimulated and much is being learned about its purely mechanical properties.

MECHANICAL PROPERTIES OF THE EAR

Impedance measurements.—The impedance characteristics of the ear determine its ability to absorb or reflect sound at the various frequencies and the extent to which these two properties manifest themselves is reflected back onto the source presenting the sound to the ear. Thus different ears react differently when coupled to an earphone, and careful measurements of this by Burkhard & Corliss (17) show that deviations of as much as 20 to 30 db may be produced in the sound pressure at the entrance to the ear merely by the acoustic termination presented by the ear.

Benson & Eldredge (14) measured in animals the changes in sound pressure produced for frequencies in the range of 50 to 8000 c.p.s. by different conditions of the drum membrane: (a) normal, (b) hair line slit, (c) rupture, (d) fluid behind membrane, and (e) open bulla. Measurements were made in front of the tympanic membrane and in a speculum conducting the sound into the ear canal. When the ratio of sound pressure levels at these two points was plotted against the frequencies used it was found that the normal ears produced the greatest variability. They conclude that the impedance of the auditory system is as important as the difference in size and external configuration of the ear canal in accounting for variations of sound pressure for various frequencies.

Many attempts have been made in the past to measure the impedance of

the normal ear. These have been reviewed and plotted by Wever & Lawrence (196) with the conclusion that the ear as a whole absorbs most readily the frequencies around 1350 c.p.s. This measure was the most accurately determined by Metz (98) who used an acoustic bridge which consists of a tube terminated by the ear at one end and a material of known impedance, whose absorption and phase changing characteristics can be altered by a piston arrangement, at the other end. Co-workers of Metz are now finding measures of impedance of the ear a useful clinical device. Thomsen (167) has used it to measure eustachian tube function and has put the changes in impedance of the ear produced by the intra-aural reflexes to use in determining other auditory activities. He reports the detection of a case of psychogenic deafness (165) and demonstrates the "Metz recruitment test" which depends upon lowered threshold level of intra-aural muscle contraction in recruiting ears (168). In another article Thomsen reviews the principles and applicability of impedance measurements (166).

Middle ear muscles.—A most comprehensive study of the acoustic stapedius reflex has been carried out by Jepsen (63). In this monograph he describes the acoustic impedance bridge as a reliable device for determining the existence of paralysis of the stapedius muscle in cases of dysfunction of the facial nerve.

The way in which these middle ear muscles alter the impedance of the middle ear and the efficiency of their actions have been subjects for investigations off and on for some time. Recently, however, a number of investigations give us much more information on their action. In a two-part paper Totsuka, Nakamura & Kirikae (174) and Okamoto, Sato & Kirikae (114) report the use of electromyography in the studies of the auricular and tensor tympani muscles in the guinea pig. In the first part there may be some confusion since the authors note that the auricular muscle is innervated by the facial nerve and state that the pathways are similar to those of the tensor tympani, and yet it is recognized that this latter muscle is innervated by the Vth cranial nerve. Obviously the authors must mean the very earliest parts of the pathway from cochlea to muscle reflex.

In the second part the reflex action of the tensor tympani itself is described. As measured from the electromyogram the latency of the burst action in the reflex was measured as 6 to 8 msec., the latency for muscle contraction as 7 to 10 msec., and for inward movement of the tympanic membrane as 18 to 19 msec. They attribute the fluctuation of the tympanic membrane under the influence of continuous loud sounds, described some time ago by Lorente de N6 (91) and others, to synchronous discharges of all the neuromuscular units of the tensor tympani muscle rather than as a tetanic contraction as previously supposed.

Wever, Vernon & Lawrence (202) measured the maximum strength of the tympanic muscles in animals by means of a strain gauge. They found large individual differences with a mean for the tensor tympani of 3.5 gm. when connected to the ossicular chain and 4.4 gm. when free. For the stapedius a

strength of 1.6 gm. was found for both the restrained and free conditions. Their evidence indicates that the primary action of these muscles is to dampen the vibratory motion rather than to impose tension upon the ossicular system. Other characteristics of these reflex activities are revealed in a further series of studies by Wever & Vernon (199, 200, 201). They find that the protection afforded the ear may be as much as 20 db for the low tones but that the contractions giving rise to this protection do not take place until the stimulation is near the limits of operation of the inner ear, a level at which the inner ear is severely overloaded and in danger of serious damage.

Studies of this destruction produced in the inner ear by excessive amplitudes of sound give further understanding of the mechanical activities of the ear. Impedance of the normal ear and variations in this are not only a result of inherent differences of the ossicular system and middle ear muscle contraction but also to certain mechanical characteristics of the inner ear. These properties determine the manner in which vibratory activity is distributed to the end organ and also exert an influence on the over-all sound absorption ability of the ear as it varies with frequency.

Mechanical action within cochlea.—This distribution of action along the basilar membrane is what is thought of generally in connection with auditory theory. As has been true of earlier experiments, attempts to produce damage to the organ of Corti through the mechanical action of sound or of small probes introduced through the wall of the cochlea have produced generalized losses of hearing and nothing so specific as to indicate a Helmholtzian place theory. This is now an old story, and we can add the following to the list: Rüedi (135), Lindquist, Neff & Schuknecht (90), Alexander & O'Brien (3), Alexander & Githler (1), Sutton & Schuknecht (159), Alexander *et al.* (2), Kimura, Schuknecht & Sutton (69), and Wever & Lawrence (197).

On the basis of the accumulated evidence there is little reason to doubt that every tone causes the entire basilar membrane to vibrate but not everywhere to an equal degree. Such conclusions result from the visual observations of Békésy (8) and from the recordings of the maximum electrical response from the cochlea by Wever & Lawrence (195). Tasaki & Fernández (164) drew conclusions from their experiment that the basal end of the basilar membrane responds to both high and low tones but that the apical region is responsive only to the lows. This is, of course, only a matter of degree, and their use of "differential" microelectrodes restricting the recordings to minute areas prevents the recording of small uniform potential changes that might be present in the apical region. Present day theories of hearing are based primarily on Békésy's observations, and there is little disagreement concerning the actual pattern of response along the basilar membrane. This action is purely mechanical, and the main difference of opinion does not involve the pattern of response but concerns how any given segment of the basilar membrane gets the displacement force that makes it vibrate.

An excellent summary of this difference was presented by Licklider (84) under the heading of "transmission line theory versus parallel-resonator

theory." Békésy (10) on the basis of phase differences between the vibrating stapes and a portion of the cochlear partition believes the mechanism is not a parallel-resonator system. The argument can be reduced to a question of just what characteristic of the displacement picture one is considering, and in 1954 Wever, Lawrence & Békésy (198) published a joint paper trying to explain this.

It was established that Wever & Lawrence in rejecting the traveling-wave theory on the basis of earlier experimentation were not implying that there was anything at fault in Békésy's observations. There is complete agreement in the patterns of response and Békésy, in making up a demonstration, coupled a row of individual resonators (pendulums and reeds) together and showed that each element vibrates but that the phase of neighboring elements is slightly different and that because of this and the fact that each element is coupled to the next a traveling wave can be seen when the elements are not considered separately but as a combined ribbon-like unit. These appear to travel always in one direction: from "base" to "apex" independent of the locus of the driving vibration. It is possible to interpret this action in two ways: one can say that each element responds because its neighbor on the basal side has pulled it, through its coupling, as a wave travels from base to apex, or one can say each element responds because it itself is driven by a displacement force applied to it, and since it is coupled to a neighbor which is also driven by this same displacement there will be a mutual influence. All these mutual influences will add up to look like a wave that is traveling along the elements always in the same direction.

Békésy (12) speaks of a paradoxical direction of wave travel along the cochlear partition because if the vibratory force is put in at the apex the wave travels toward this source. He believes that each "element"⁴ of the basilar membrane gets its displacement force from its neighboring "element" on the basal side and that no matter where the sound is introduced into the inner ear fluid system the alternating pressure wave in the fluid ignores all intervening "elements," or vice versa, and proceeds to the basal end to start the wave traveling. According to Wever & Lawrence there may be a point of introduction of the vibratory displacement, but the actual source of vibration is in the fluid surrounding all "elements" practically at once. No matter where the vibration is introduced it goes through the fluids more or less with the speed of sound in water, thus surrounding all "elements" practically simultaneously with an alternating displacement force. These "elements" responding in their own way are all coupled together, and the resulting pattern is exactly the same as shown in Békésy's observations. The

⁴ The term "element" is used only for convenience and to keep the discussion consistent with the resonator demonstration of Békésy. No anatomical division into elements exists. The basilar membrane is a ribbon-like structure bounding one side of the cochlear partition whose properties vary from one end of the cochlea to the other much as in the manner of the model described by Békésy (13).

difference, then, is really one of interpretation as to how an "element" gets its displacement force.

A crucial test of these views can be found in histological studies of the organ of Corti. If part of the basal turn has some of its impedance characteristics changed such as mass (no organ of Corti) or stiffness (calcification of basilar membrane), a system which has each "element" depending upon its neighbor in the basal direction for the displacement force will not work in the same way; the pattern would have to be radically changed or eliminated. If the basal region can be altered with the rest of the system working the same way, an interpretation other than that of traveling waves is necessary. If the view is taken that the wave starts further toward the apex under these altered conditions, one must concede the influence of the displacement force mediated anywhere and everywhere through the fluids. There are many examples of animal and human ears in which the organ of Corti has disintegrated in the basal region and yet the perception of low tones remains unaltered. Guild (52) reports a case in which an aberrant blood vessel was attached to the basilar membrane and anchored to the bony spiral lamina and the bony septum between the apical and middle turns of the cochlea. Despite the damping this must have provided, the hearing tests prior to death showed normal hearing for low tones.

The next question of theoretical interest concerns the manner in which pitch can arise from such nonspecific activity along the basilar membrane. Except for the low frequencies which initiate synchronous neural discharges, thus giving a direct number for pitch, the problem is no longer mechanical. The structure of the organ of Corti and its physiological performance must be considered since more than just the pattern of vibration along the basilar membrane is involved in this sharpening mechanism.

PHYSIOLOGY OF THE INNER EAR

The sharpening mechanism.—As the basilar membrane is vibrated the sensory cells of the organ of Corti are distorted. This action is associated with an electrical potential change, and in a recent review article Davis (25) agrees that these hair cells are the source of the potentials and recognizes that they probably are the mechanism by which the nerve impulse is initiated. Curiously, however, he states that pitch is determined by the extent of basilar membrane activated from the basal end. He maintains that as frequency is lowered, more and more of the basilar membrane proceeding from base to apex is activated and that for each frequency there is a "sharp boundary between an excited region and the unexcited region." This sharp break in activity is the cue to pitch, he says. In 1900 ter Kuile (73) had a theory similar to this but he did not have the advantage of Békésy's observations.

Békésy never observed such a sharp cut-off in activity, and in order to investigate the nature of the sharpening mechanism he constructed an ingenious model (13) which gives the same pattern of activity as conceived

for the basilar membrane. A cylinder filled with liquid has a slot down one side covered with a plastic membrane. Pulses of sound are introduced into the liquid and a pattern of response is set up on the membrane. If one places the forearm or any other accessible part of the body along this membrane, the impulse is felt at only one point: the region of maximum displacement along the membrane. Response to the other regions is somehow inhibited, which Békésy attributes to the nervous system. There is a question of whether this inhibition occurs at the skin or centrally. If one places both hands on this model the maximum amplitude inhibits the sensation at the other position. This would indicate that the phenomenon is a central one and if applied to the cochlea would indicate an ability of the nerve fibers to relay in temporal fashion the characteristics of the basilar membrane pattern.

There are other characteristics of cochlear action that may be related to this problem. In 1949 Wever (194) postulated the presence of a cathodic summation occurring at the terminal fibrils of the auditory fibers. If a stimulus pulse fails to excite a nerve fiber it nevertheless leaves behind a "local potential" and a "local response" [see Katz (68)]. The negative aspect of this pulse has more effect in neutralizing the resting potential around the fibril than the positive; so the local charge builds up. The more rapid the pulses the quicker this charge reaches a magnitude sufficient to cause the fiber to fire. This sort of activity would lower the threshold for high frequencies.

In 1950 Davis, Fernández & McAuliffe (26) reported the presence of such a potential. This was observed as a shift in the base line of the cochlear potential response to tone-pips of 8000 c.p.s. Most recently Goldstein (50) has subjected this summing potential to further analysis, but the exact relationship between this and nerve stimulation has not yet been demonstrated.

Various resting potentials have been described for regions of the cochlea. Békésy (11) has reviewed his observation of them. Recently Tasaki, Davis & Eldredge (163) explored these again and found that increasing the potassium content of the scala tympani abolished the cochlear A.C. potential and the nerve responses. On the basis of this they suggest that the reticular membrane rather than the basilar membrane is the dividing surface between endolymph of the scala media and perilymph of the scala tympani [see also Davis (24)].

That this activity within the cochlea is an electrochemical one has again been demonstrated by a clever histochemical study by Churchill, Schuknecht & Doran (23). They demonstrated the presence of acetylcholinesterase within definite regions of the organ of Corti of the cat.

Factors influencing cochlear activity.—That the study of cochlear action is not simple is evidenced by the many factors that influence its function. The injection of KCl as reported above gives evidence of complications, but even more natural phenomena affect the response. Gisselsson (49), Fernán-

dez (42), and Tonndorf, Hyde & Brogan (173) have again demonstrated the effects of oxygen deprivation upon the cochlear potentials.

The way in which Ménière's disease influences cochlear function is still a mystery. One theory maintains that the blood vessels responsible for the production of endolymph are under such autonomic control that if this system fails in any way in its normal local action a labyrinthine hydrops is produced which, by the increase in pressure, causes a loss of hearing. Furstenberg (45) discussed this problem at some length, and Lewis (82) reported some relief in 11 out of 18 patients by employing cervical sympathectomy. Perlman & Kimura (119) observed the minute cochlear vessels of the cat and guinea pig through a small window made in the bony wall lying over the spiral ligament. No visible changes in these vessels or the blood flow were found on electrical stimulation (6 v. at 60 c.p.s.) of the stellate ganglion, cervical trunk, superior cervical ganglion, vertebral artery, basilar artery, or anterior inferior cerebellar artery. Brief stimulation of the stellate ganglion and cervical sympathetic trunk produced a prompt effect upon the middle ear vessels together with a slowing, followed by complete arrest except for to and fro pulsations of blood in the mucosa over the cochlear capsule. With prolonged stimulation this effect disappeared. They do not say what effect this latter procedure had on the cochlear vessels. Krejci & Bornschein (72) using cats could not change the magnitude of the cochlear potentials by prolonged sympathetic stimulation. This is in direct opposition to results reported earlier by Seymour & Tappin (143).

The opposing concept of the cause of loss of hearing in Ménière's disease assumes a toxin within the endolymph (81). Seymour (142) argues against this with histopathological evidence, and Tumarkin (175) theorizes that recruitment (associated with Ménière's disease) can best be accounted for on the basis of the accumulation of endolymph within the scala media.

That recruitment is indicative of a lesion within the inner ear is evident from the accumulation of clinical evidence. Yantis (206) has reviewed this evidence and shown that, where the cochlea is not involved, lesions of the auditory nerve or more central pathways do not give rise to the recruitment phenomenon.

The nerve pathways and higher centers.—Although the activities of the peripheral mechanism of the cochlea can be studied visually and electrically, their perceptual manifestation cannot be understood until there is a more thorough understanding of the nerve and its more central connections.

Tasaki & Davis (162) have continued research on individual nerve elements by means of microelectrodes, and many studies have been carried out on the characteristics of the cortical responses [Lilly & Cherry (89); Lowy & Coleman (92); Perl & Casby (118); Pribram, Rosner & Rosenblith (130); Rosenzweig (133); Rosner & Heise (134); Tunturi (176, 177)]. A good deal of the earlier work and the goals aimed for in research on the higher centers are covered in a review by Rosenblith (132). In this he makes a plea for the use of clicks in auditory research. Techniques have not yet been devised that

can successfully reveal the activity of the higher centers during stimulation of the auditory system by a sustained pure tone, although this does elicit a behavioral response in the conscious organism. A most excellent and thorough review of the present status of anatomy and theories of function of the auditory nerve and higher centers is given by Galambos (46).

Some very interesting clinical observations have been made in connection with the neural pathways. Bordley & Haskins (16) report studies on patients who can hear but do not understand what they hear. These patients showed an elevated audiometric threshold but a significantly better threshold when measured by the psychogalvanic method. The investigators feel they have measured the efficiency of the end organ and subcortical auditory pathways by the psychogalvanic method and that the subjective method represents the performance of an inadequate auditory cortex. Tunturi (176) found evidence for this in experiments on dogs in which he removed the suprasylvian gyrus bilaterally with no permanent effect on leg responses conditioned to sound and tactile stimuli.

Other instances of patients who hear but cannot discriminate well are reported by Schuknecht & Woellner (140) and by Walsh & Goodman (181). In both reports, individuals with acoustic neurinomas showed fairly normal pure tone audiograms but gave poor discrimination scores on speech tests.

INTENSITY DISCRIMINATION AND LOUDNESS

Intensity discrimination in the recruiting and normal ear.—Recruitment, a phenomenon arising perceptually from a disturbance of end organ function, has been defined as a more rapid growth of loudness in the abnormal ear than in the normal. One way of demonstrating this has been to compare the loudness of a tone in one ear with that in the other; another is to compare the loudness of one tone in an ear with a loss for that tone with the loudness of another tone for which this same ear has no loss. Because of the nature of the losses these procedures cannot always be carried out, and it has been reasoned that the difference limen for intensity should be a way of detecting recruitment. If loudness grows more rapidly in the deficient ear the difference limens should be smaller. Lüscher (94) has reviewed this technique using amplitude modulated tones for finding the just detectable difference limen and insists it is a valid technique for measuring recruitment. There are, however, two reports that question this procedure, although neither method employed the amplitude modulation technique.

Hirsh, Palva & Goodman (56) in a thorough, well-done study explored the relation among results of loudness balance, speech, and intensity difference limen tests obtained on 44 patients with different types of hearing loss and 18 normal hearing individuals.

The intensity difference limens were obtained at sensation levels of 5, 25, and 40 db at the same frequency used for the measuring of recruitment by the alternate loudness-balance method. The tones were presented in pairs: one held at a constant intensity and the other varied in 1 db steps from +5

db to 0 db (the level of the constant tone) and, alternately, from -5 to 0 db. There were a few cases in which a difference of greater than 5 db was necessary to establish an initial discrimination. Ten judgments of equality were made, and the difference limen was taken as the difference in decibels between physical equality and the nearest intensity difference that gave a subjective judgment of "different." The results of this indicate that when recruitment accompanies a hearing loss for speech of more than 20 db there will be an associated low discrimination score, but that in general whatever relation does exist between the size of the DL and recruitment is so dependent upon the procedure used that any one technique for measuring the DL is not reliable for clinical use.

Jerger (65) carried out the same sort of experiment on two groups of 10 subjects each: one group with normal hearing at 1000 c.p.s. and 4000 c.p.s., and another with normal threshold for 1000 c.p.s. but with various degrees of hearing loss accompanied by recruitment at 4000 c.p.s. (as determined by the monaural loudness matching procedure). The DL was determined by periodically adding short intensity increments to the standard tone which was presented at two base intensities: 10 db above threshold, and 40 db above threshold. From this, psychometric functions were constructed. The results showed that the patients with the 4000 c.p.s. hearing loss and recruitment had a much smaller DL than the control subjects with normal hearing at this same frequency; but on questioning the experimental subjects after each run it was revealed that they experienced shifts in the loudness of the reference tone over the 2 min. that the tone was kept on. No such changes were reported by the control observers. On the basis of this Jerger suggests that the smaller DL at 4000 c.p.s. in the experimental group is related not to the slope of the loudness function but to this shift in the ear's response to the sustained stimulation.

It is quite understandable that confusion should exist in attempts to relate the intensity difference limen in abnormal ears to the loudness function when the difference limen in the normal ear varies so with the technique of measurement as does apparently the loudness function.

Pollack (124, 125, 126) in a most ambitious series of experiments shows how this DL can vary with experimental conditions, and Tonndorf, Brogan & Washburn (172) have made similar observations in a large number of normal hearing subjects. Small & Thurlow (146) have related certain masking effects to recruitment.

The loudness function.—DLs do not provide a scale of loudness in that the loudness of a tone one DL removed from another may not have the same subjective ratio value as a pair of DL-separated tones at some other point in the intensity or frequency range. The psychological question has been whether a scale of loudness can be constructed such that it could be predicted that a tone of a given intensity would sound twice (or any other ratio) as loud as another tone of set intensity. This is really a very challenging question; it taxes the skill of the psychologist at his best; it has been demonstrated

as well as in any other area investigated that results depend upon the method of investigation used. But the fact remains that one can experience a loudness change as intensity is increased and the question is: has the psychologist the skill and technique to quantify this? The recent reopening of the problem of developing a loudness scale has stemmed not from the discovery that any loudness scale is invalid but from a realization that there are many technique-borne artifacts that must be eliminated or at least recognized.

Garner (47) has demonstrated that the context of the experimental situation may have an effect on the outcome of loudness scale construction. He has raised an interesting question in connection with this demonstration. It may be, he says, that an observer has a perfectly good scale of loudness in his sensory system, but he may not be able to align this with the numerical rules that the experimenter sets for him. This can be put to test by presenting different ranges of stimuli to the observer. If he can very easily convert his loudness experience into a numerical scale he should have little difficulty for any range of stimuli presented to him, but if he cannot readily relate his loudness to any numerical assignment it should be possible to present different ranges of stimuli and have him accept them as satisfying the stated numerical requirements. In this experiment the purpose was to determine to what extent half-loudness judgments determined by the method of constant stimuli could be influenced by the context of stimuli presented to him. The constant reference tone was set at a sound pressure level of 90 db, and the comparison tone was presented at a level which varied over a 10 db range. There were three ranges: 55 to 65 db, 65 to 75 db, and 75 to 85 db. The highest range included the half-loudness value expected from the present sone scale of loudness. Each of the observers was satisfied to accept some value within the particular range presented to him as being half as loud as the constant tone. This point was not significantly different from the mid-point of the particular range of variable stimuli. The experimenter's conclusion is that observers find themselves unable to assign a scale of numbers to their sensations.

Another of the disturbing facts in the accumulated data on loudness scales is that the experiment by Laird, Taylor & Wille (74) produced results which have not been confirmed by results from later experimenters. In an investigation of this in which the experiment was repeated, Stevens, Rogers & Herrnstein (157) found that if the comparison tone was placed far below, i.e., much less intense than the standard tone (as much as 40 decibels), the results obtained were similar to those of Laird, Taylor & Wille in that a reduction of 20 db was judged as half as loud, whereas, by not reducing the comparison tone as much, the customary value of 10 db reduction for half-loudness is obtained.

What this indicates is that in order to quantify this loudness sensation, context effects must be designed out of the experiment. Stevens (154) presents a nice philosophical discussion of the goals and pitfalls in procedures designed to establish a loudness scale. In order to avoid the context effect

and any influence that a numerical scale assigned by the experimenter might have, it is possible to present the tonal stimuli and ask the observer to indicate the apparent ratio between them. This process Stevens calls the method of magnitude-estimation. The standard tone is assigned some convenient number, 1, 10, or 100, for example, and the observer's task is to assign a number to the variable so that it indicates the subjective ratio between the standard and the variable. Another method for eliminating the factors introduced by an experimenter-assigned numerical scale does away with the standard tone altogether, and the observer assigns whatever number seems appropriate to designate the loudness of a series of intensities.

These two procedures give results consistent with the pooled results of a number of other investigators and indicate a loudness scale that is a power function of physical intensity: $L = kI^{0.3}$.

A review of available evidence both published and unpublished on the relation between loudness and intensity was reported by Stevens (153). Here the above equation is further explained: "In terms of sones where one sone is the loudness produced by a tone at 40 decibels above the standard reference level, the equation for loudness L as a function of the number of decibels N becomes: $\log L = 0.03 N - 1.2$." A pair of intensities differing by 10 db gives a loudness ratio of 2:1, and this appears to hold true over the entire range of audible intensities. A reservation should be made, however. At extremely high intensities there are indications that loudness may no longer increase, in fact it may decrease. At these levels experimentation is hazardous because of the possibility of damage to the sensory structures, but an earlier report by Békésy (7) indicates that the loudness may not continue to rise.

Poulton & Stevens (129) found loudness relations for white noise. The median decibel change necessary to produce a 2:1 loudness ratio of this noise ranged from 6 to 10 db. In a recent note Stevens (155) gives a method for calculating the loudness of noise.

A very interesting approach has been utilized by Stevens & Poulton (156). They found that 65 unpracticed observers were able, on their first judgment, to estimate loudness ratios consistently. This was true for both of the procedures used: a method of adjustment and the method of magnitude estimation. The use of a "sone potentiometer" in which loudness is proportional to degree rotation of the control knob rather than intensity as has been the usual practice produces the best results. Another method based on the statistical manipulation of a rating scale for comparing loudness has been demonstrated by Michels & Doser (105).

As was evident from the discussion of recruitment one cannot dwell long on the problem of loudness without encountering the problem of fatigue, and during the past two years this has been a very popular subject for investigation.

FATIGUE

Traditionally auditory fatigue has been measured in terms of an elevation in threshold, but it has also been clearly shown that measures above this

level show a change in loudness for the stimulated ear when compared to the other ear if the measurement is made during stimulation. These different types of measurement have brought forth some very curious facts and continued research should be very rewarding.

Poststimulation fatigue.—The use of the automatic audiometer described by Békésy (9) has proven ideal for tracing the fatigue effects of different frequencies at various intensities and durations, and for plotting a continuous picture of the recovery from this fatigue. It may be some time before a complete picture can be put together, but when that time comes these studies will have contributed a lot to our understanding of how the ear works.

Reger & Lierle (85, 131) have traced the threshold shift following stimulation by sensation levels of 20 and 80 db. Using 21 subjects, a test frequency of 1000 c.p.s., and an exposure time of 1 min. they made the observation that the mean threshold shift for a 20 db level stimulation was 0.28 db greater than that for an 80 db level. The standard deviations for the distribution of threshold shifts were 1.66 db for the 20 db stimulation level and 3.0 db for the 80 db level. Marked individual differences were characteristic of these studies. If the stimulus was kept on for a half hour the fatigue effects were greater for the 80 db fatiguing tone than for the 20 db tone.

Hirsh & Bilger (55) have verified these results with no more than a 4 db greater shift following the low level stimulation. This is a thorough study using the Békésy audiometer to investigate the magnitude and course of recovery of the temporary hearing loss following exposures to pure tones of moderate intensities. In one experiment they subjected the ears of their observers to 1 min. exposures of 1000 and 2000 c.p.s. at sensation levels of 10, 20, 40, 60, 80, and 100 db. Threshold shifts and recovery pattern were recorded at 800, 1000, 1200, 1400 and 1600 c.p.s. for a stimulus of 1000 c.p.s. and at double these frequencies for a stimulus of 2000 c.p.s.

From the results of this series they concluded that there is very little difference in the amount of threshold shift produced by different intensities up to 100 db above threshold for stimulus tones of 1000 and 2000 c.p.s., but the recovery of the threshold is more rapid following intense exposures than following the exposure to the weaker tones. As the fatiguing tone is increased in intensity the threshold shift for a frequency of one-half octave higher increases.

Another study was carried out using only 1000 c.p.s. at sensation levels of 20, 60, 80, 90, and 100 db for durations of 10, 15, and 30 sec. and 1, 2, and 4 min. Thresholds were recorded continuously following stimulation for frequencies of 1000 c.p.s. and 1400 c.p.s.

Following a 1000 c.p.s. exposure at sensation levels of 60 db or higher the threshold for 1000 c.p.s. showed a rapid recovery followed by a slower recovery process or even a second rise in the threshold. This latter effect Hirsh & Ward (58) have termed the "bounce." Following exposure to a 20 db tone there is no similar rapid recovery and this, they say, explains the

apparently greater shift following a 20 db exposure than following one of 80 db.

The effects are somewhat different when threshold recovery for 1400 c.p.s. is followed after exposure to 1000 c.p.s. In this case the temporary hearing loss increases directly with duration of fatiguing tone for sensation levels of 80 db or more and increases directly with level for sensation levels greater than 60 db.

Studies of fatigue effects raise many important and critical questions. When Caussé & Chavasse (20) made their excellent study of fatigue following low level stimuli they expressed the opinion that much of the earlier confusion resulting from fatigue studies lay in the fact that tones of too great an intensity had been used, and what they called fatigue was being confused with early stages of stimulation deafness. This is still a problem and most investigators studying fatigue at the present time feel compelled to analyze their results into different recovery processes, but in most cases the research design is such as to reveal nothing more than a picture of threshold loss and recovery in a general way.

Jerger (66) considered this difficulty and designed a piece of research to test a specific notion. He noted that, as shown by Hirsh & Ward, the pattern of recovery following a fatiguing stimulation consists of a threshold lowering toward its original level but showing a second rise, or bounce, at approximately 2 min. after cessation of the fatiguing stimulus. The maximum fatiguing intensity in these investigations had been a sensation level of 120 db. On the other hand Harris (53) using fatiguing intensities of 120 to 140 db above threshold failed to produce this bounce and maintained that in other experiments it was attributable to a tinnitus arising as a result of the overstimulation. Jerger designed his experiment to investigate the possibility that a critical factor in the appearance of this secondary maximum in the recovery curve might be the intensity of the fatiguing stimulus. He presented a 3000 c.p.s. fatiguing tone for 2 min. and followed the recovery of the threshold for 4000 c.p.s. When a stimulus sound pressure level of 95 db and above was reached there was a sudden jump in the amount of initial hearing loss accompanied by a gradual decrease in the amount of bounce. At a sound pressure level of 110 db this secondary maximum had just about disappeared from the recovery curve. He suggests that this disappearance of the bounce may mark the transition from fatigue to early reversible stimulation deafness.

Jerger (64) reports a second study designed to get at this same problem. This was based on the well-established fact that there is a critical duration of tone for threshold stimuli below which intensity must be increased to maintain the threshold response. This critical duration has, in different investigations, been reported to be between 125 and 200 msec. Below this critical duration the intensity of the stimulus must be increased to establish a threshold response. Garner & Miller (48) have suggested, on the basis of their research, that the threshold intensity in decibels is a logarithmic function of

stimulus duration. Jerger calls attention to the observation of Miskolczy-Fodor (107) that, in ears with organ of Corti type deafness, this intensity-duration relationship does not hold in that the intensity level required for threshold does not increase as the stimulus duration is decreased to the same degree that is required in the normal ear. This makes possible a test of the locus of the fatigue effect in the normal ears. So Jerger set up his experiment to expose normal ears to thermal noise at a sound pressure level of 110 db for 2 min. and then to study the resultant threshold for 4000 c.p.s. short tones of three different durations: 500, 50, and 5 msec. These short tones were presented as a train of impulses, one every 1.25 seconds, and the observer by means of a Békésy type attenuator control bracketed the intensity necessary to make the tones audible. The results indicate that after exposure a greater intensity is required to maintain threshold for a tone duration of 500 msec. than for that of 50 msec., and similarly a greater intensity is required to maintain the threshold for 50 msec. tones than for 5 msec. This is the same as Miskolczy-Fodor found in his organ of Corti type hearing loss cases. The conclusion is that this stimulation by noise at a sound pressure level of 110 db has involved structures of the organ of Corti.

In all of these studies the amounts of threshold shift and bounce vary considerably from individual to individual, and a consistent pattern is not always produced. Lightfoot (87) undertook to investigate the extent of these individual differences and was able to produce a bounce in some individuals and no bounce in others. He found that his observers differed greatly in general fatigability and recovery rate but generally remained in the same rank order during the recovery process.

Another poststimulatory effect that occurs under certain conditions of stimulation is sensitization: an improvement in sensitivity or a lowering of the threshold for certain tones. Hughes (61) found it possible to demonstrate a mean lowering of the threshold of 6 db 70 sec. following exposure to a 500 c.p.s. tone at 100 db above threshold for 3 min. This sensitization brought about by 500 c.p.s. stimulation spread over a wide range of frequencies, the greatest effect occurring for frequencies above that of the stimulation tone. Also a wide range of stimulating frequencies sensitizes the ear to 500 c.p.s. The maximum of this effect was 4 db.

One other study of poststimulatory effects is of interest. Lightfoot & Jerger (88) measured the effects of 2 sec. bursts of thermal noise on the threshold for 35 msec. tone pips. Reasonably enough they found that increasing the sound pressure level of the bursts produced greater fatigue.

Perstimulatory fatigue.—Carterette (19) performed an experiment similar to the one above using auditory localization as a measure of the fatigue. Of extreme interest in terms of workings of the ear are the studies related to the nature of fatigue when measured in this manner.

In 1949 Dix, Hallpike & Hood (29) and a little later Hood (59) described a method of using the binaural intensity cue for localizing sound as a method for measuring fatigue in one of the ears under test. In this technique one of

the tones is fixed in intensity and presented to one of the ears of the observer.

Another tone which is variable in intensity is presented periodically to the other ear during periods of stimulation in the test ear. The observer, controlling the variable intensity stimulus, makes a loudness balance which results in a tone localized in a plane midway between the two ears. They have called this "perstimulatory" because the fatigue is measured while the sound producing it remains on. As pointed out by Carterette in the article just cited this sort of fatigue measure assumes that the condition of the control ear, the one to which the periodic variable matching intensity is presented, remains unchanged during the course of the experiment. Users of this technique feel justified in this assumption because of the slow onset of fatigue when measured in this manner.

Many interesting observations have been made using this technique. Hood, using pure tones, found it was necessary for observers to decrease the intensities in the control ear by as much as 35 to 40 db to keep the tone localized in the median plane. Though fatigue of this sort starts slowly, in 3 to 4 min. the amount of shift reaches its maximum which varies directly as a function of the sound pressure level of the fatiguing tone. If fatigue is measured by the alternate loudness balance method after the fatiguing tone has been turned off, a poststimulatory measure, no evidence of a shift in sensitivity of the fatigued ear can be demonstrated [Davis *et al.* (27)] unless this measure is made within 1 sec. of the cessation of the stimulus. In experiments in which this was done [de Maré (95); Wood (205)] fatigue effects as detected by the alternate binaural loudness balance method were found.

Hood has explained these apparently contradictory observations by resorting to the work of Matthews (97) on stretch receptors. This investigator showed that when a stimulus is applied to one of these receptors the associated nerve fiber gives an initial burst known as the "on-effect." This lasts for only about 0.2 sec. and is followed by a slow decline in frequency of discharge. The receptor and nerve apparently do not recover from this until about 1 sec. has elapsed; during this time a diminished "on-effect" can be produced. A stimulus applied sooner than 0.2 sec. after application of the initial stimulus produces no effect. This is why, according to Hood, perstimulatory fatigue or alternate binaural loudness balance made immediately following cessation of the fatiguing tone shows 30 to 40 db of threshold shift whereas poststimulatory measures of loudness balance made after 1 sec. show no effect. Why threshold measures made continuously by the Békésy audiometer after cessation of a fatiguing stimulus may show effects in the form of a loss of sensitivity for minutes following the stimulation is a question unanswered by Hood's analogy.

It is worthy of note in this regard that Egan & Thwing (34) measured very little fatigue using an alternate binaural loudness balance method in which the tone was rapidly alternated between ears so that each received the tone for 0.5 sec. over a period of 15 secs. Of course, if there were no pre

vious monaural stimulation to fatigue one ear and not the other, one would not expect any difference to show up between ears, each being stimulated the same amount of time during the balancing procedure. The description of this experiment is not clear, but it is assumed there must have been some prebalance monaural stimulation.

Hood (60) undertook to make further measures of this in a procedure similar to that described above. He found that if he went back to a binaural localization procedure following an alternate binaural loudness balance procedure to measure fatigue as the result of previous stimulation, a shift in the loudness level (localization) was immediately apparent even though none had been evident through measures of the alternate binaural matching method.

Egan (30) wished to test whether or not the method of localizing the sound in making a simultaneous loudness balance is critical to the occurrence of perstimulatory fatigue. To answer this he performed an experiment in which a loudness balance was made between tones of different frequency, in which case the sounds were localized in the individual ears, the observer hearing two pure tones. The results from this procedure, when compared with those determined by the use of the localization technique, showed no essential difference, demonstrating that the localization of the tone as a technique is not a critical or essential aspect of perstimulatory fatigue.

In another very interesting experiment Egan (31) compared the perstimulatory fatigue resulting from a noise of uniform spectrum between 100 and 5000 c.p.s. with the fatigue of a tone just heard above this noise. As was to be expected with continuous 90 db sound pressure level noise in one ear it was necessary to reduce the level of the comparison tone by an intensity amounting to 16 db in a period of 7 min. On the other hand while this continuous noise produced this fatigue in the ear the intensity level of a 1000 c.p.s. tone necessary to be audible through masking noise remained unchanged. There may be some question as to whether these results are comparable since one measure is binaural and the other monaural, but the results are puzzling.

It is interesting to know how much perstimulatory fatigue at one frequency will affect the loudness of surrounding frequencies when measured by the same method. Thwing (170) made measurements of this for frequencies centered around 1000 c.p.s. and found perstimulatory fatigue following the presentation of 1000 c.p.s. at a sound pressure level of 80 db for various short periods of time. Tones from 1000 c.p.s. to 5000 c.p.s. were affected with the maximum effect occurring at frequencies nearest the fatiguing tone.

There have been several clinical applications suggested for these procedures based largely on the fact that they correlate somewhat with some other effects rather than that there is any understanding of the phenomenon. Hood (60) for instance has found that if one employs an intermittent loudness balance method in a recruiting ear the recruitment is readily observable, but

if the simultaneous binaural loudness balance (localization or perstimulatory) technique is used the recruitment effect is abolished. Lansberg (75) and Palva (116) have also discussed some of these possibilities.

Occasionally it is found in certain ears that a gradual increase in intensity of a steady stimulating tone is necessary to maintain the listener's audibility threshold. This has been termed "threshold fatigue," and Lierle & Reger (86) and Kos (71) have associated this with the presence of VIIIth nerve tumors. But there has been considerable evidence that such threshold fatigue occurs in instances of so-called end-organ deafness. The experiment of Jerger's (65) reported earlier suggests this, and Hood (60) has even described a test for Ménière's disease based on this in which the threshold is first obtained in the conventional manner, then the tone is raised 5 db and maintained continuously until the subject no longer hears it. In subjects with end-organ deafness the tone becomes inaudible in 5 or 6 sec. The tone is then raised another 5 db and so on. The normal ear does not exhibit this phenomenon but gives rise only to a persistent tonal sensation.

Stimulation deafness.—At the other extreme of the intensity range it is possible to produce permanent damage, often referred to as stimulation deafness. Except for some experimental work on animals the most prevalent research interests have been of an actuarial nature in which amount of hearing loss for large groups of workers in a certain noisy environment is calculated. Webster (185), Webster & Solomon (188), Pestalozza (120), and many others have reported studies of this nature. There have been few studies on the factors determining injury in the individual ear. Rüedi (136) in studies on the effects of vitamin A has recommended it as a panacea for the relief or prevention of almost all end-organ afflictions including the toxic effects of streptomycin and neomycin, and states that individual tolerance to loud sounds can be increased by 10 to 15 db through the use of the vitamin. The evidence upon which this is based is not very convincing.

Harris (54) has reported that there is a cumulative effect following fatigue even though threshold has returned to normal. He has referred to this as "latent damage" and interpreted it as being a result of changes in the ganglion cells, but there is just as much evidence for placing the effect in the sensory cells.

FREQUENCY DISCRIMINATION AND PITCH

Problems related to the perception of pitch have not been ignored, and the influence of pathological conditions has received its share of interest, even though our understanding of human abilities to discriminate frequency or to construct a subjective scale of pitch is not as well established as it is for those related to intensity.

Frequency range.—An interesting experiment related to the range of frequencies over which the ear can respond was carried out by Deatherage, Jeffress & Blodgett (28). They report that during one summer while working in water in the beam of a 50 kc. transducer a sound was heard. The sound

could only be heard by bone conduction, and the investigators believe that the auditory sensation is the result of direct stimulation of the basal end of the cochlea. They base this conclusion and another, that the experience is not attributable to creation of a sonic frequency vibration of the head, upon two most interesting observations. (a) The pitch heard is actually the highest pitch the person can hear, i.e., if the frequency is emitted in the ordinary audible range and then raised there is a high frequency beyond which pitch does not change even though the frequency of the transducer continues to rise. (b) As the frequency is changed in this ultrasonic region changes in localization of the sound within the head occur. This, the authors explain, is probably a result of the change of standing wave patterns in the head. The same shifts in localization occur also by holding frequency constant and changing the point of contact between the transducer and the head.

When the jaw is slightly immersed in a bucket of water containing the transducer a threshold stimulation is reached at 2000 dynes/cm² for 50 kc. If the entire body is immersed in the water containing the sound field, threshold is reached at 1000 dynes/cm². The investigators warn anyone trying this experiment to be careful of the amount of sound used. Although pain is seldom experienced, a long lasting tinnitus may result which is possibly a sign of inner ear damage.

In this connection it is interesting to note the experiment of Neff & Hind (113) in which they found evidence in cats of hearing up to 60 kc. This was carried out by the conditioning method and with air conducted sound. At 60 kc threshold was reached at about 100 dynes/cm².

Diplacusic.—In the experiment of Deatherage, Jeffress & Blodgett just described it was noticed that the pitch sensation was not always the same in the two ears. This they attribute to diplacusic, a phenomenon which has been receiving considerable attention, perhaps because of improved techniques for its measurement. The causes of diplacusic are unknown but since the phenomenon is generally more marked in ears with some form of pathology, studies have been carried out in an attempt either to use the presence of diplacusic as a diagnostic device or to use the presence of known pathology in the exploration of the causes of diplacusic.

Strange (158) has reported an experiment in which she studied the effect of intensity on pitch in subjects with a dip between 3000 and 6000 c.p.s. in the audiogram of at least one ear. She found that in 15 out of 17 individuals pitch changed with intensity most markedly in the region of the hearing loss. There was no consistency, however, in the direction of pitch change for different subjects or for different frequencies in the same ear.

Webster & Schubert (186) created artificial threshold shifts by the use of masking with narrow bands of noise. Using an automatic audiometer they obtained a running record of pitch matches between the two ears. The tendency revealed was for a pitch to shift away from a region of the masking noise to a region in which the masking effect was less or absent. In three cases with a hearing loss for frequencies above 500 or 1000 c.p.s. the fre-

quency shift was always downward or away from the region of loss. They interpret their results in terms of "tuned" elements and include a good review of the literature on the subject of diplacusis.

Christman (22) carried out a similar experiment producing threshold shifts by prolonged stimulation with pure tones. The results were much the same as Webster & Schubert's but the interpretation is quite different in that a "figural after-effect" theory is suggested.

Another form of diplacusis has been described in which a pure tone appears as double, atonal, or noisy. Ward (184) describes two cases of tonal monaural diplacusis in which the ear responds to a pure tone with a sensation of several distinct tones quite far apart in pitch. By using an exploring tone to match with the extra subjective tones it appeared that the additional tones corresponded to combination tones expected to be produced in a normal ear if stimulated by a tone of a fixed frequency and a variable one corresponding to those used in this experiment. In other words, a pure tone introduced into these ears produced a sensation of pitch corresponding to the one presented, a subjective tone of a fixed frequency (independent of the stimulating frequency), and a series of combination tones. These are interesting observations but unfortunately this type of monaural diplacusis is extremely rare.

Frequency discrimination.—A factor to be considered but one that has received little attention in the study of binaural diplacusis is the accompanying existence of an impairment of the frequency discrimination ability. Failure on the part of the observer to make close pitch matches could be accounted for by this factor alone, and investigators of diplacusis have not always considered this possibility. Butler & Albrite (18) have shown how extensive this loss of discrimination ability can be, and Meurman (99) has proposed a specific frequency difference limen test for diagnostic purposes.

Three other studies dealing with frequency discrimination should be mentioned. Pikler & Harris (121) studied abilities to discriminate frequencies when a number of different channels were used. When conditions in different channels are matched they give equivalent results except for channels such as "successive interaural" in which diplacusis can exert an effect. The results are also quite different in pathological ears.

Bachem (4) tested what he has called time factors in relation to frequency discrimination. His experiment was based on the notion that those individuals possessing the ability of absolute pitch [reviewed in another paper (5)] could retain a pitch in memory more accurately than could those individuals not possessing this ability. The experiment deals more with pitch memory than frequency discrimination. Of 10 subjects, 5 with the absolute pitch ability and 5 without, there seemed to be little difference in recalling a previously presented pitch if the time lapse between judgments was short, but for longer time intervals those possessing absolute pitch were more

accurate providing the tones were within the musical range. Baker & Osgood (6) studied ways of training the ability to discriminate frequencies.

Pitch scales.—In reviewing the interpretations that have been placed on various results obtained from experiments on pitch perception in abnormal ears it does not appear that much has been settled. This no doubt is, in large part, attributable to the fact that the phenomena as they occur in the normal ear are not well understood or even established. There is no question but that diplacusis affects pitch discrimination or that poor pitch discrimination may be interpreted as diplacusis. Diplacusis appears to be more common than once supposed which may account for the variability found in establishing frequency discrimination limens and pitch scales, but such subjective measures have been made. Stevens (152) has recently considered anew two matters of concern in connection with pitch perception: (a) is there a fixed relationship between difference limens for frequency and units of a subjective pitch scale and (b) are the original, generally accepted, data of Shower & Biddulph (144) in error, because, as Kock (70) contends, a frequency modulation technique was used? Stevens argues that a just noticeable difference does represent a constant number of subjective pitch units if these latter are properly established, and that Kock's use of modulation theory to explain Shower & Biddulph's results is invalid.

Ward (183) has tackled a problem concerning subjective pitch scales that has long been left untouched. He conducted a series of experiments to determine a scale of subjective musical pitch. Specifically he tested the ability to estimate octaves. Here again there was found to be considerable interobserver variability and significant day to day variation in judgments. In some instances it could be shown that diplacusis was responsible for the variability, but this could not explain all the variations.

OTHER SUBJECTIVE ATTRIBUTES

Aural harmonics and combination tones.—Meyer in a number of articles (101 to 104) recently has been defending his hydraulic, nonvibratory interpretation of cochlear activity by indicating the combinations of tones which are capable of producing a "Tartini pitch." Sandstad (138) has pointed out that Meyer's results can be explained by assuming a "nearly symmetric nonlinearity in the mechanical transmission of sound in the ear." When two pure tones of frequency f_1 and f_2 act upon a system that responds in a nonlinear manner a complex vibration consisting of many components results. These components can be expressed by the relationship $f_x = \pm mf_1 \pm nf_2$ where m and n are whole numbers indicating harmonics of the pure sinusoidal driving frequencies. The intensity of these components depends upon the kind of nonlinear distortion exhibited by the system. If it is symmetrical, i.e., if the tops and bottoms of the driving sine wave are both flattened out, the result will be a production of odd harmonics, and if two sine waves drive this system, odd-order ($|m| + |n| = \text{odd number}$) combination tones will

result. If this occurs in the ear the odd-order distortion products will be perceived as louder than other components. This, according to Sandstad, accounts for the presence of the "Tartini pitch" as described by Meyer without other components being readily audible.

There have been many studies made of these distortion characteristics of the ear both psychophysical and electrophysiological. The most recent and thorough treatment of this subject appears in Wever & Lawrence (196), and probably the most significant conclusion reached after analysis of old and new experimental evidence is that this nonlinear distortion does not occur in the middle ear. The middle ear responds linearly up to amplitudes of vibration that would have already destroyed the inner ear. And apparently alterations of the middle ear through disease processes do not change these linear characteristics.

Lawrence & Yantis (79) report a series of measurements in subjects (a) with normal hearing, (b) with pure conductive loss, and (c) with cochlear involvement. Using the exploring tone method they found the level of both a 1000 and 2000 c.p.s. tone to which each must be raised above threshold to produce a just detectable second harmonic. This they call the threshold of aural overload and the level of this point above threshold as the linear range of the ear. In a group of normal ears they found the mean linear range at 1000 c.p.s. to be 52 db with a standard deviation of 13 db and a linear range at 2000 c.p.s. to be 57 db with a standard deviation of 17 db. This range, despite varying amounts of threshold elevation, remained essentially the same in individuals with impairment of the middle ear mechanism. In ears with cochlear involvement, however, this range was reduced to 24 db and a standard deviation of 14 db for 1000 c.p.s. and 18 db and a standard deviation of 10 db for 2000 c.p.s.

Results quite similar to these have been reported by Opheim & Flottorp (115). These investigators used lower frequencies of 250 and 500 c.p.s. which are in good agreement with the lower frequencies used in an earlier report by Lawrence & Blanchard (77). In this later investigation it was suggested that the linear range of the inner ear reflects its physiological condition: the shorter this range the poorer the condition. This suggestion is convincingly proven by the results of Lawrence & Yantis and Opheim & Flottorp.

Sokółowski (149) used an exploring tone introduced by bone conduction to measure the presence of the aural harmonics. Those abnormal ears that have the inner ear involved aside from exhibiting a short linear range also show recruitment. Because of this Sokółowski recommended the use of the early appearance of harmonics as a measure of recruitment.

Although it has not been investigated, the distortion characteristics of the ear may be related to the phenomenon of diplacusis. Lichte & Gray (83) have shown that overtone structure influences the pitch of complex tones so it is not too unreasonable to suppose that the overtone structure in an overloaded ear would influence the pitch of the perceived tone. Zwicker (207) and

Zwicker & Spindler (208) have also made quantitative measurements of the ear's distortion characteristics.

One big advantage of using the distortion characteristics of the ear or the linear range in diagnostic procedures is that it is quite independent of the middle ear condition. As is well known and recently demonstrated again by Palva & Ojala (117) and Goodhill & Holcomb (51) bone conduction can be considerably altered by middle ear lesions thus giving a false picture of the inner ear condition, the so-called cochlear reserve. Use of the linear range permits fairly accurate quantitative measure of inner ear function.

Auditory localization.—Recently the influence of arrival time on auditory localization has been emphasized by several investigators. Snow (148) presents data collected in the Bell laboratories in 1934 and now reported for the first time. Differences in arrival time of from 0 to 30 msec. of the various pulses making up a complex sound have a great influence on the apparent source of the sound. At present this is a disconcerting obstacle to practical stereophonic localization because these arrival times differ and hence so does the apparent position of the sound source.

Fay & Hall (40) have straightened out the history on the directional illusion associated with time delay pointing out that it was mentioned in the 1856 Annual Report of the Smithsonian Institution by Joseph Henry. Mouzon (110) and Mathes (96) report on monaural localization, a discussion which is difficult to understand from the standpoint of the psychology of hearing. It appears to be more a problem in learning than hearing. Other papers on auditory localization that have appeared are those of Sandel and co-workers (137), Wilcott (203), Wilcott & Gales (204), and Meurman & Meurman (100).

Interrupted stimuli.—Two phenomena important to our understanding of the ear's operation that are produced by the interruption of sounds are the rate at which these sounds fuse and the perception of tonality at the interruption frequency.

Symmes, Chapman & Halstead (160) have reported on auditory flutter fusion using intermittent bursts of white noise. The observation that an interrupted sensation decays linearly with time and does so over a fixed period of time regardless of intensity level leads these investigators to write a generalized equation for the auditory flutter fusion function,

$$f = \frac{I \times c}{\Delta I \times T} \times 1000:$$

where f = repetition rate at the threshold of fusion, I = sound level in decibels above threshold, ΔI = the intensity DL for noise in decibels, T = the duration in milliseconds of the psychological decay to threshold, c = 1.0 minus sound-time function.

Mowbray, Gebhard & Byham (111) measured the sensitivity of the ear to changes in the interruption rate of white noise. Their results indicate a

relative difference-limen ($\Delta F/F$) of between 0.008 and 0.03. This is smaller than that previously reported by Miller & Taylor (106), and the authors point out that there is a difference in method. Mowbray, *et al.* were also unable to obtain reports of tonality as the noise was interrupted at the various rates (1 to 320 c.p.s.) whereas Miller & Taylor reported such pitch characteristics in their observers for interruption rates up to 200 c.p.s.

More thorough studies of this apparent pitch have been carried out by Small (145) and by Thurlow & Small (169). These investigations expose interesting theoretical problems but must await further experimentation before discussions are more than academic.

HEARING IN COMMUNICATIONS

The subjects covered in this review have been primarily those that give us some further information on how the ear works or are, at least, closely related to this policy. The problem of hearing and its properties as part of a communication system is a subject in itself and many advances have been made.

In the area of speech reception Thwing (171) has reported an experiment demonstrating that articulation scores for PB words are slightly higher if the words are repeated twice. Black (15) explored the monaural intelligibility scores for nonsense syllables delivered by two channels simultaneously with specific amounts of delay in one of them. For other aspects the reader is referred to other studies (57, 109, 123, 127).

A number of studies have also appeared on the subject of message reception and ways of improving this (21, 32, 33, 128, 139, 150, 151, 187, 189, 190).

Two separate groups of investigators, Munson & Karlin (112) and Tanner, Swets & Green (161), have explored the problem of signal detection in noise, referring noise to either a physiological background or external noise. Both show that ability to detect is a function of the attitude or set of the observer depending upon how familiar he is with what to expect or how anxious he is to give a satisfactory performance. There are some observers who are able to perform more effectively, and Munson & Karlin suggest that it should be possible to select individuals with low physiological noise levels as well as with sensitive detection ability.

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SOMESTHESIS^{1,2}

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Somesthesia is a word which veils "in the decent obscurity of a learned language" a number of sensory functions diverse as to their relations to conscious experience, their dependence upon, and independence of, one another and other senses, and the character of their contributions to the economy of the organism. Sherrington's framework of classification brings some, though no final, order into this limbo. The cutaneous senses, for instance, may often be employed in perceptual activity directed upon objects in the environment and in that connection are exteroceptive. But such activity would very often be incomplete and abortive without the co-operation of other organs and afferent pathways, notably those of muscle and joint, which in other connections are properly spoken of as proprioceptive. At other times the sensory functions of the skin are concerned not with objects in the environment, but with physical conditions prevailing in the neighborhood of the skin itself or in its own substance such as the temperature of water, the movement of air, or the dryness of the epidermis. Here external skin is performing a function not very different from that of some membranes inside the body. Exteroception approximates to interoception. Those organs, again, which are most commonly and characteristically concerned with proprioception perform a number of services. They, or some of them, make us directly conscious of the attitude of the body or of the position of parts of it, and control the movements whereby other sense organs are brought into effective play and used for exploration. But the central nervous system also gains from them information by means of which the impressions transmitted by these other sense organs are transformed and ridded of features introduced by the varied positions and movements of the sense organ itself.

Schiller (1), in a long review of these and related problems of classification and description viewed from a variety of directions, would propose a number of changes in our traditional conceptions and ways of talk. Thus he points to the spatial-localization aspect implicit in a number of somesthetic functions which cuts across distinctions of organ, pathway, and modality. He remarks that

it seems expedient to gather together the spatial aspects of perception as they are given in touch, proprioception (including sense of balance), kinaesthesia, two-point discrimination, stereognosis, and vibration into one "function": "locoception," for want of a better name. There is, however, no reason to deny to fibres of small size,

¹ The survey of the literature pertaining to this review was completed in April, 1956.

² The following abbreviation is used in this review: EEG for electroencephalogram.

or to the anterolateral tract all ability of conveying information as to the where and when of stimulation, including size of area and repetition of stimulus.

Some may feel that this unification of terminology would be achieved only at the expense of a narrow abstraction. The fact that all of these functions do and must operate in terms of systems of determinate spatial variables hardly justifies us in regarding their functions as primarily "lococeptive." But, armed with a sufficient and growing body of empirical data, there is certainly useful scope for further discussion of such problems.

In trying to pick our way through this maze of interrelated organs, pathways, and functions, the worse confused by its seemingly inconsequential traffic with conscious experience, the threads of adaptive reaction are doubtless in the long run the safest to follow. The simpler models presented by lower animals (2, 3, 14) are astringent and suggestive and, it may be hoped, will rapidly multiply in the immediate future. But for the present a division of topics partly in terms of function and partly in terms of the prevailing occupations of active workers seems the only one practicable.

MODALITIES, PATHWAYS, AND PATTERNS

As readers of previous reviews will have noted, a prevailing tendency of the past 10 years in the study of body sensibility in general, and cutaneous sensibility in particular, has been away from the classical framework of qualities of sensation and types of sense organ. This has amounted to closer attention being paid to the system of free nerve endings and its physiological possibilities of mediating a variety of determinate sensations. On the other hand, the problems of the encapsulated organs have been, at least temporarily, relegated to future study. The general view that differing patterns of excitation in the interwoven naked endings which innervate an area of skin may to a large extent account for the intensities, qualities, and local signs of sensations aroused by stimulating it has gained ground largely through the work of Weddell, Sinclair and their associates. The scope of the evidence as it stood two years back was indicated by Weddell's review of 1955 (4). In the period at present under consideration much of this, and further evidence, has appeared in a more extended definitive form. The word "pattern," for instance, has been provided with a concrete connotation and need no longer alarm those who are shy of such conceptions. The year's output in this field is a rich one and includes surveys of the problems and evidence by Sinclair (5), by Weddell, Palmer & Pallie (6), and, in more abstract vein, by Schiller (1). The massive contributions by Weddell, Pallie & Palmer (7), Weddell & Pallie (8), and Weddell, Taylor & Williams (9) incorporate the clearest formulation of a "pattern" type of theory yet available.

Sinclair's (5) advocacy of the pattern theory largely takes the form of a thorough-going critique of classical views which took their stand upon faith in end-organ specificity and the adequacy of existing denotations of sensation quality. He discusses evidence from all fields and concludes,

The factors disclosed by histology run counter to the idea of the morphological differentiation of specific endings, and the physiology of undifferentiated endings appears adequate to subserve most sensations. The adherents of von Frey have now no anatomical evidence on which to pin their specificity except the varying size of cutaneous nerve fibres.

Sinclair continues,

It seems to be established that what leaves the skin as a result of cutaneous stimulation is a complex spatially and temporally dispersed pattern of impulses, and this, or something like it, appears to be what arrives at the sensory cortex of the brain, though the vicissitudes which it undergoes in the spinal cord and ascending pathways have not yet been worked out. It is claimed that the physical characteristics of this pattern are the determining factor in the experience of sensory quality.

To some this statement may appear provocative in the light of the present data. But a number of this year's contributions are at least consistent with it. The general principle, for instance, that the same network of nerve endings may subserve sensory reactions to a variety of different mechanical stimuli is illustrated by the work of Passano & Pantin (3) on the sea-anemone *Calliactis parasitica*. The endodermal nerve net of this animal is responsive to mechanical deformation. But the thresholds, and the reactions evoked to stimuli applied to different parts of the integument, are various and can be shown to depend not upon the properties of the sensitive organs but on the morphological organization of the body as a whole and especially on the mechanical structure in the neighborhood of the point stimulated.

As to the morphological variety and specificity of organs found in the human skin Weddell *et al.* (6) provide in a convenient summary a discussion of the very varied conclusions reached by previous generations of histologists. Examination of the drawings published by these workers suggests, it is held, that there was less variety in what they saw than in the descriptions they gave, often in terms contaminated by gratuitous presupposition. Other discrepancies may be accounted for by the occurrence of artifacts which the use of new techniques now allows to be discriminated from true appearances [Weddell, Pallie & Palmer (10)]. It emerges from the application of these techniques that in hairy human skin there are only free nerve-terminations and, specifically in relation to hair follicles, unencapsulated endings. This is borne out by Dastur (11) who, in the course of his study of over 200 specimens of hairy skin from many parts of the body in a number of leprosy patients, found only free and hair-follicle endings. In glabrous skin (from the lips, anus, glans penis, etc.) numerous encapsulated endings occur, but their morphological variety is continuous, and no distinctive types exist. Weddell *et al.* (6)

are forced to the conclusion that there are not and never have been any purely *histological* grounds on which to erect theories of cutaneous sensibility based on the existence of four primary modalities, touch, warmth, cold and pain, operating within the "law of specific nervous energy." Furthermore there is not and never really has

been convincing histological evidence for the commonly accepted statement that morphologically specific nerve endings subserve each of the primary modalities of cutaneous sensibility.

Weddell *et al.* (7, 8, 9) bring a number of important aspects of cutaneous sensibility to a focus in terms of the anatomical and physiological paradigm which is provided by the ear of the rabbit. The argument of these three papers requires close attention and cannot be adequately summarized in brief. The evidence put forward, which is anatomical, histological, and electro-physiological, must be omitted. The chief points, however, are as follows. There are about 100,000 hairs on the ear of the Dutch rabbit, and slightly less than this number of follicles. These 100,000 hairs emerge from about 40,000 orifices, and the latter are arranged in about 13,000 groups. The number of myelinated fibers entering the base of the ear to supply these hairs is of the order of 6,500. Thus there is on the average only one fiber per two groups of orifices, one per six orifices and about one per 16 hairs. These fibers, of course, divide and subdivide, but not in such a way that contiguous hairs are subserved by the branches of a single fiber. If they did so divide, and discrimination of points stimulated were to be supposed to depend upon fiber-pathway differences in the afferent trunk, the system would allow a two-point threshold of about 7/10 mm., this being twice the average spacing of groups of orifices, two groups of which, as stated above, on the average share one fiber. In fact the area of skin subserved by branches of one fiber can be shown to be large compared with a square millimeter, and these areas overlap one another. This is established by a study of the degeneration consequent upon section of fasciculi of the afferent trunk, together with electro-physiological mapping of the overlapping areas of skin, stimulation of which evokes action potentials in the fasciculi.

It is clear, then, that the fiber composition of the afferent trunks affords no guide to the spatial discrimination capacity of the sensory system. The situation is, in fact, that each area of skin is subserved by branches from a number of different afferent fibers. A further complication arises in that each follicle is found to have associated with it terminations from at least two and generally more (up to 30) trunk fibers, the average number being about four to five. Bearing in mind that there are about 16 hairs to each fiber in the afferent trunk, this means that each such fiber innervates between 60 and 80 hairs. The question therefore arises whether, if a single hair be stimulated, sufficient information in the form of unique patterns of excitation in the nerve-trunk is transmitted to decide which of the (say) 80 hairs subserved by a single fiber it is.

The authors point out (though in terms of a somewhat sketchy application of information theory) that the combinations of "on" and "off" in a set of six elements will afford over 60 ($<2^6$) distinct combinations, while seven will provide over 100 ($<2^7$). They appear to wish to suggest that something not far short of these six or seven independent elements may be provided by the terminations on a follicle from (an average of) five distinct trunk fibers.

But for this suggestion to be justified would require each of the 80 hairs which can discharge down a single trunk-fiber to be innervated by a different selection of five or so other trunk-fibers. It does not seem clear that the facts ascertained by the authors regarding the distribution of fiber branches go so far as to guarantee this.

Though it may be possible to challenge their conclusions at a detailed level and to wish for a steppingstone from the rabbit's ear, about whose discriminatory capacity little is known in terms of behavior, to the human skin, Weddell *et al.* have clearly rendered valuable service in bringing the pattern theory of cutaneous discrimination down to the level of concrete, if sometimes conjectural, discussion. Nor do they have to rely solely on the possibilities of patterns in terms of simultaneous excitation in independent fibers. The fibers vary in diameter and accordingly in conduction-speed, and this offers another dimension to increase the information-capacity of the channel, though the authors do not pursue this aspect in quantitative detail.

Sinclair (5) urges that differing conduction-speeds should not be taken, as has sometimes been held, to imply that the corresponding fibers subserve different modalities. Discussing this question he notes also that "Reaction-time measurements have in the past provided one basis for the idea that specific sensory modalities are carried in specific ranges of fibre sizes to the central nervous system, and that the 'pain fibre' is distinguishable from the 'touch fibre' by its size." In studies of conditions which affect reaction times to thermal stimuli Lele & Sinclair (12), following previous similar work on touch by Lele, Sinclair & Weddell (13), find not only considerable dependence upon the intensity of the stimuli but also variation from day to day, from site to site, and from subject to subject. They conclude, as had Lele *et al.* (13) in regard to touch, that it is not legitimate to base a comparison of conduction-velocities (and hence a distinction of modalities) upon reaction time measurement.

The work of Dodt (14) affords a specific example of the simultaneous activity of myelinated and unmyelinated fibers in response to a cutaneous stimulus. This worker, using a method of stimulation devised by Hensel, Ström & Zotterman (15), applied thermal changes to two sites on the frog, namely the lingual mucous membrane and the dorsum. In each case action potentials were recorded from both myelinated and unmyelinated fibers of the afferent pathways. He found responses to heat stimuli in both types of fiber in the case of the tongue but only in unmyelinated pathways in the case of the skin of the back. Characteristic differences were found between the responses in the two types of fiber when the mucosal tissue was stimulated. The response in myelinated fibers, occurring after a latency of 1/10 second, showed rapid onset, then a slow decrease; and on maintaining a constant raised temperature no lasting discharge was seen. The maximum sensitivity of the system lay between 10°C. and 30°C. On repeated stimulation there was considerable diminution of excitability. A clear diminution of activity was produced by 2 per cent KCl, and procaine (novocaine) abolished

it after a short time. By contrast, the latency in the system subserved by non-myelinated fibers was of the order of seconds, the onset of activity slow, and with maintained stimulus the discharge continued. Repeated stimulation involved no diminution of excitability. Stimuli of less than 30°C. produced only small activity. There was no effect with 2 per cent KCl, and procaine exercised an influence only after long application. The characteristics of the discharge in myelinated fibers on similar stimulation of the skin of the back were similar to those found in myelinated fibers subserving the tongue.

In the context of a discussion of "modalities" various interpretations could be placed on these results of Dodt. It might be said, for instance, that they demonstrate the existence of two separate "modes" distinguished by stimulus (*viz.*, different effective temperatures), by pathway (type of fiber), by rate of adaptation, and by susceptibility to chemical agents. The skin of the back possesses one of these modalities and the tongue both. Opponents of the "modality" conception might, on the other hand, maintain that the difference between the effects of stimulating tongue and back consists only in a difference of spatio-temporal patterns of excitation in the afferent pathways, factors of structure and function upon which the pattern-difference is based being irrelevant to what is essential, namely the conveyance of information. Others again, considering the notion of "modality" to be heavily impregnated with the idea of quality of experienced sensation, might consider it inappropriate in relation to Dodt's experiments on the frog and prefer to view his results in terms of the functional adequacy of two sensory systems to bring about the responses the animal may be called on to make to differing stimulus situations. The human being whose sensory experiences, though evident to him, seem to provide a lamentably diffuse basis for the classification of sensory function, might perhaps take a hint from the frog.

Two further investigations published during the past 12 months bear upon Sinclair's (5) formulation of the problems of human cutaneous sensibility. If sensations of various qualities and intensities are mediated by the stimulus of a network of interlaced free nerve-endings in the skin, the failure of some of these elements to conduct should bring about a change, qualitative, quantitative or both, in the impressions received. Instances of such effects were brought forward a number of years ago by Weddell and his associates, especially in connection with the protopathic-epicritic controversy.

Latterly Dastur (11) and Gilliatt (16) have reported further data in this connection, and they agree in concluding that the system of terminations in normal skin incorporates a considerable reserve of capacity and is proof against some loss of functional elements. Dastur, after clinical tests for touch, warmth, and pain, carried out biopsy of vitally stained (methylene blue) skin areas from many parts of the body on 224 leprosy patients. His failure to find encapsulated organs in hairy skin has already been mentioned. He notes further that damaged hair follicle endings often look like some encapsulated organs from glabrous skin.

Dastur's specimens included a variety of degrees of destruction in the plexus of free nerve endings, the amount required to occasion sensory loss according to his tests being considerable. "Peripheral sensory innervation" he remarks "carries a considerable 'safety factor'." As the disease marches centripetally, warmth and pain are first affected, touch later. This he attributes to the superficial position of the structures responsible for pain and warmth relative to the deeper-lying hair follicles, which his cotton-wool touch test would chiefly stimulate.

Gilliatt (16) has investigated, in 11 patients with sensory loss on the hand attributable to spinal or cerebral lesions, the effect of occlusion of the circulation by pneumatic tourniquet. He finds that on the affected side the loss of sensation attributable to the occlusion is added to that consequent upon the lesion, and by application of the cuff small losses, undetectable by ordinary means, can be brought to light, and the progress of sensory recovery or deterioration can be more precisely followed. He infers, like Dastur, that there is a margin of reserve in the system which in practice offsets some degree of anatomical or physiological breakdown. The use of occlusion in cases of sensory loss as a result of central or peripheral lesions would seem to offer considerable scope for investigation, using more precise laboratory test techniques.

The locus of time-dependent sensory processes such as adaptation has been much discussed in the last decade, largely in connection with evidence from brain-injured patients. Jaffe (17) reports data relating to the adaptation time for the stimulus of a pin prick (loaded with 8 gm.). He compares the times on the affected and normal sides in brain-injured patients suffering sensory loss with those in subjects with sensory defect as a result of peripheral nerve injuries. The stimuli on the two sides were given both separately and simultaneously. He finds that 5/15 of the brain-injured on single- and 13/15 on double-stimulation showed diminished adaptation-time. Among the cases of peripheral nerve-damage, only 3/10 showed any asymmetry of adaptation time. Jaffe concludes that cutaneous adaptation is a function of central processes. Infiltration of the peripheral pathway with novocaine immediately after the onset of stimulation could, of course, also curtail the adaptation time, but one would hesitate to say on that account that cutaneous adaptation is a function of processes in afferent nerves. The greater proportion of cases showing the effect on double- than on single-stimulation suggests a more complex origin for it.

PERCEPTION

Studies of perceptual and motor activity in which the somesthetic sensory systems play a prominent, and often complex, role have not been numerous during the year under review. Weinstein (18, 19, 20) has reported a series of experiments on size and weight judgments in brain-injured patients. In the first of these (18) he is concerned with the tactile perception of the size of the base of a metal cylinder using the single-stimulus method. Four groups

of 15 subjects were compared: (a) brain-injured with somato-sensory impairment of the hand, (b) brain-injured without such impairment, (c) peripheral nerve-injured (arm), and (d) peripheral nerve-injured (leg). Judgments by both successive and simultaneous comparison were obtained. Only in the brain-injured patients with somato-sensory impairment did a significant difference of judgment between the two sides emerge, and then only with simultaneous comparison. The object was judged smaller on the affected side. Effects of possible differences of temperature and thermal sensibility between the two sides are not discussed by the author. Comparing the results of this experiment with those of an earlier one the author (21) finds the effect of a cerebral lesion to be more severe upon weight- than on size-perception, at any rate when tested by simultaneous bilateral stimulation. He accordingly disagrees with Head's finding that in lesions of the sensory cortex weight- and size-perception are disturbed in equal measure.

Using the same groups of subjects Weinstein (19) found significantly greater negative time errors for weight-perception on the unilateral stimulation of either the normal or the affected hand than on bilateral stimulation. In general the effect of a lesion of the parietal lobe was to produce a negative time error greater than that found in control and in subjects with lesions in parts of the cortex other than the parietal lobe. But this increase of the negative time error was found to be the same for the affected as for the normal hands. The meaning of these results would seem to require further study. A similar experiment [Weinstein (20)] on the time error in size judgment performed on the same subjects again showed a significantly greater time error on unilateral than on bilateral stimulation. Subjects with somato-sensory defect performed the same as those without. Frontal lobe cases presented anomalies by comparison with all others. Weinstein's results as a whole incorporate complexities not all of which reflect necessarily upon the specific cortical mechanisms of size- and weight-perception.

The study of subjective scales of sensation has been continued by Baker & Dudek (22). In an experiment involving the impaired comparison of weights, the sum of the two weights being in all cases constant, they obtained judgments under two conditions of constraint imposed on the form of report. In the first the subjects had to state the relation between the two weights as a direct ratio of heavier to lighter. In the second they were called upon to divide 100 points between the two weights in such a way as to represent their relationship. The functions relating judgments to corresponding physical values were found to differ in these two cases. Both also differed from the functions previously derived by other workers. The evidence that the precise task imposed upon the subject exercises an effect upon the relationship of physical stimulus and sensory response would by now seem to be sufficiently conclusive!

Study of the part played by somesthesia in the direct control of movement continues, chiefly through experiment on the manipulation of controls. Bahrack, Fitts & Schneider (23), for instance, have investigated the stability

in respect of spatial and temporal uniformity of movement patterns acquired by practice using visual and auditory guides. After this practice the subjects had to repeat the movements under various conditions of altered elasticity and inertial and liquid damping of the lever which had to be moved. They found that changes of elasticity had no significant effect on spatial accuracy. Increase of damping, however, whether viscous or inertial, produced greater uniformity of speed. Weiss (24) also finds that the correction of an oscilloscope spot after displacement during a period in which no visual clue is given is not affected as regards either its constant error or its precision as measured by percentage accuracy.

Provins (45) has studied the respective roles of skin pressure-receptors and muscle- and tendon-receptors in the task of reproducing a prescribed pressure by the index finger under isometric conditions. Comparing normal performance with that obtained when the digital nerve was blocked by α -diethylamino-2,6-aceto-xylicide (Xylocaine), thus removing the afferent contribution from the skin, he finds that where the force to be applied is less than 400 gm. the skin receptors make a considerable contribution, whereas above this level their role is of little importance.

PROPRIOCEPTION, ORIENTATION, AND AWARENESS OF THE BODY

Increasingly adequate knowledge is forthcoming regarding the workings of proprioceptive sense organs and of the basic forms of reaction by which parts of the body are made to move and take up positions relative to one another in a coordinated and stable fashion. But the manner in which the sensory products of these activities are utilized by the central nervous system to orientate and otherwise adapt perceptual and motor activity to an independent external world remains obscure. Not less difficult are the related problems of the way in which information from the proprioceptive systems is elaborated, often in shadowy and vacillating form (and subject to bizarre disturbances in pathological conditions), into a conscious image of the body and its situation.

In the field of the sense-organs themselves the outstanding event of the year was unquestionably the publication of Granit's Silliman Lectures (25). No brief review, even of the relevant parts of this book, could do justice to it, and it must be simply mentioned, as must be the valuable contribution of Cooper, Daniel & Whitteridge (26) summarizing evidence accumulated by them to establish the presence of stretch-recorders in the human extrinsic eye muscles. A long standing doubt of some consequence is thus resolved.

A communication by Cohen (2) is valuable as adding to the collection of invertebrate sensory systems which may serve as models for speculation and experiment on higher forms. He reports that the statocyst receptors of the lobster, *Homarus americanus*, are capable of transmitting information of four different kinds, namely (a) of absolute position, (b) of absolute position and the direction from which it is approached, (c) of angular acceleration, and (d) of vibration transmitted through the shell.

Turning to the central aspects of some of the more specific proprioceptive reactions, we may note Carmichael, Dix & Hallpike's report (27) of the effects of localized unilateral cortical lesions upon the caloric and opto-kinetic reactions. In a group of 73 cases they find that large lesions of the posterior parieto-temporal region cause directional preponderance of both caloric and opto-kinetic nystagmus to the affected side. The two reactions may, however, be disturbed independently, and the area concerned with the caloric response is found to lie in the posterior part of the temporal lobe, while that for the opto-kinetic lies in the region of the angular, and supra-marginal gyri. It may be hoped that the coordination of such findings with the disturbances of visual and topographic disorientation sometimes produced by lesions in the same regions may point the way to fuller understanding of the proprioceptive framework of perceptual processes. Another interesting point regarding vestibulo-optic interactions is reported by Graybiel & Patterson (28) who find the oculo-gyric illusion to be an astonishingly delicate indicator of otolith responses to changes in the gravitational field. They established a mean threshold in three subjects of 3.44×10^{-3} g.

One aspect of the role of proprioceptive factors in activity demanding conformity to a spatial frame of reference is illustrated by the experiments of Sandström (29) on the effect of tilting the head upon errors in tactile adjustment of a rod to the vertical position. He finds that the errors, which differ significantly between the two sexes, in general amount to a deviation opposite to the direction of head tilt. This effect is interesting as being opposite in direction to the Aubert effect obtained when the adjustment is carried out by visual means, and Sandström notes that it is more clear cut.

Weiner (30) has studied the effects of training upon the capacity of subjects to adjust into a vertical orientation a cubiform frame of luminous lines or a luminous rod situated within the cubic frame. The training consisted in experience of different attitudes of the body, the exercises being carried out by a tape-recorded commentary. It appeared that directing the subjects' attention in these exercises to the bodily sensations obtained when the body was tilted led to a significant improvement in their capacity to adjust an object into a vertical direction.

Finally we should note one or two contributions during the past year bearing upon more general aspects of the awareness of the body. The experiments of Roger, Rossi & Zironoli (31) perhaps contribute something to the old problem of the dependence of consciousness upon streams of continuous afferent activity, especially those deriving from proprioceptive sources. These authors studied the dependence of the EEG pattern of alertness in "encéphale isolé" cats upon various afferent sources. They found no marked modification of the EEG pattern when in turn the olfactory, visual, acoustic, vestibular, and vagal sources were suppressed. Bilateral destruction of the Gasserian ganglion involving suppression of trigeminal influences, however,

led to the appearance in the EEG of the sleep rhythm. To that extent a muscular source for the afferent supply which contains wakefulness would seem to be borne out.

Lastly Benton (32) has reported upon disturbances of right-left discrimination and finger localization in defective children. He finds a significantly greater incidence of poor performance among defective subjects than among normal children of the same mental age. The type of defect, whether "familial" or "brain-injured," appears to be immaterial, but there is a small but statistically significant association between the two disturbances. He thus fails to find evidence of the involvement of specific cortical mechanisms, but it remains possible that the general disturbance of intelligence from which both groups of children suffered had masked disturbances of more specific origin which might have been present.

PAIN

During the period under review, perhaps the most notable event in the study of pain was the publication of White & Sweet's extended treatise (33). That the work is primarily concerned with the neurosurgical alleviation of otherwise intractable pain by no means renders it uninteresting or unimportant to the more general student. Called in very often as a last resort, the neurosurgeon's attack is with few exceptions limited to the discovery of points in the pain pathways at which interruption by section or a chemical agent effects abolition of pain without too grave a sacrifice of physiological function or of other modes of sensibility. Such procedures afford brilliant success in a number of cases and encourage the surgeon to further practical study of the pathways and to fresh experiment in operation, with results of continuing interest. No less interesting are the reflections aroused by partial or complete failure. The very success of some operations in which permanent alleviation, with little physiological disability, is obtained, for instance, by severing the pain pathways where for a space they run segregated in the antero-lateral tracts of the cord, throws into relief not only the extreme complexity of their course at both lower and higher levels but also those aspects of pain-experience which have implications beyond the simple conceptions of "modality" and "pathway."

The authors, assisted by contributions from the pen of Dr. Stanley Cobb, accordingly pay considerable attention to the role of individual and psychiatric factors in the production and perpetuation of pain. When an operation gives virtually complete relief to one patient and partly or wholly fails in the case of another, a number of factors may account for the difference. The pathway-interruption may have been unintentionally incomplete. There may be important individual variations in the fiber distribution and in particular in the place of decussation. The extent and the character of the disease giving rise to the pain may differ or follow different courses of progression subsequent to operation. But the authors' large experience by no

means leads them to discount the suggestion often made on other, sometimes less concrete, grounds that the arousal and continuance of pain is to an important extent determined by factors of individual personality, temperament, and history, and of the cause of the disease. Thus they stress the damaging effect of repeating a procedure (such as the excision of a neuroma) which has failed to give more than temporary relief, and the desirability of undertaking sufficiently radical surgical treatment before the patient's condition has deteriorated through drug or alcohol addiction.

As to the cerebral mechanisms of pain, upon and in terms of which personal and historical factors might be expected to exercise their effect, the findings of surgical adventure cast disappointingly little light. Where the pain fibers of the spinal and medullary tracts lose themselves in the mid-brain, the surgeon can go no further than the histologist in saying what becomes of them. The authors are not impressed by the outcome of excision of sensory cortex from the post-central gyrus in the treatment either of "central" pain of the Dejerine-Roussy type or of painful phantoms. Apart from attendant risks such as epilepsy and dysphasia, success is limited and rarely lasting. That even temporary abolition of the phantom, and of the pain in it, can result from resection of a cortical sensory area is certainly not without interest to the physiological psychologist, however tantalizing it may be to the surgeon. White & Sweet attribute the recurrence, which generally takes place within a few weeks or months, to the impossibility of eradicating from the cortex everything that may be concerned in the sensory appreciation of even a single part of the body.

Other forms of surgical interference with the cerebral hemispheres such as leucotomy, the authors find more successful, but the mutilation of the personality, for some unexplained reason frequently severer than that produced in psychotics, calls for restraint in the practice of such operations. The extent to which they afford truly permanent relief is difficult to assess, since in many cases they are undertaken only in the terminal phases of fatal illness. It is no disparagement of the work of these authors to say that little in the way of fresh clear-cut conclusions or of striking new hypotheses emerges from their mass of skillfully accumulated and cautiously interpreted data. Every psychologist interested in pain can study this volume with profit.

Possibilities of active diagnosis and therapy greatly reinforce the theoretical need to tackle problems regarding the definitive description of types of pain-sensation and pain-reaction. At one end of the scale we have the prick of a pin, comparable in distinctiveness to the impression of a point source of light, and as easily defined so far as stimulus conditions are concerned. At the other are pains of internal causation, unknown pathology, ill-defined character and location, and widespread organic and personal interaction. In such cases it may be conjectured that the intensity of the pain "sensation" is dependent less upon the stimulus itself, than upon a mode of reaction to it.

Szasz (34) offers a discussion of these problems and distinguishes three types of pain, or as he perhaps might better say, pain-situation. In the first, pain is akin to sensation and is the outcome of a physical stimulus acting upon a normal physiological receptor system. In the second, the character and the intensity of the pain are chiefly dependent upon the reaction of the organism to the stimulus. In the third, there is specifically social involvement, the pain partaking of the nature of a "request for help." Szasz would seem to imply not that the complaint of already existing pain is used symbolically as a signal of need, but that the existence and character of the pain itself arise out of and give expression to the need.

In somewhat comparable terms Weinstein, Kahn & Slote (35) examine the problem of "pain asymbolia" in lesions of the central nervous system. In a study of 23 patients whose symptoms and EEG appearances pointed to involvement of the diffuse projection systems and "centrencephalic" regions of the cerebrum, they found degrees of asymbolia for pain which they consider closely related to the phenomena of withdrawal and inattention sometimes manifested by such patients. An apparent lack of appreciation of pain and of bodily awareness they regard as "motivated modes of adaptation to stress rather than individual defects in perceptual or motor modalities." This they conclude because the relevant behavior shows a considerable degree of selectivity. Withdrawn patients may be unresponsive only to questions about illness or hospital, or inattentive only to limbs on a disabled side of the body. In the same way lack of appreciation of the painful character of the stimulus may be shown only in certain types of situation involving a second person. The authors adduce a relationship to pre-morbid personality and, like Szasz in the opposite connection, attribute a communicational function to the symptom. They further canvass the possibility of regarding thalamic pain as an inverse manifestation of similar origin.

A possible clue to central or thalamic pain is suggested by the experiments of Spiegel & Szekely (36). They investigated the effect of ventral posterior thalamic nucleotomy in cats on the electrical activity of the corresponding parietal lobe sensory cortex. A transitory loss or diminution of spontaneous discharge in this area was followed by spontaneous activity in periods or continuously. In the latter case there were spontaneous fluctuations. At this stage, in spite of the interruption of the direct thalamic pathways, there was still response to sensory nerve stimulation. This "secondary" response was bigger on the affected than on the normal side. Spiegel & Szekely discuss the possible connection of these effects with the Dejerine-Roussy thalamic syndrome and suggest that this hyperactivity in an "isolated" cortical area may play a part in this connection. They are, however, careful to point out that it cannot afford a complete explanation since total isolation of the sensory cortex by under-cutting does not eliminate the pain or the hyperpathia.

To the extent that they suggest fresh fields for systematic observation,

discussions such as these are perhaps not without value. But present confusions about pain will, doubtless, be clarified chiefly in the measure in which the various physiological reactions involved can be revealed in appropriate and reliable experiments. Not very many such are yet to hand, but Elithorn *et al.* (37, 38) provide data relevant to one aspect of the matter. In the first of these communications the authors report a comparison of the amplitude of psychogalvanic response to pain (electric shock) with that to a warning (light) signal which precedes it in 13 psychiatric patients before and after leucotomy. They found in 12 cases an increase in this ratio, that is, a relative reduction in the anticipatory autonomic disturbance occasioned by the warning. They were satisfied that this did not arise through a change in the perception of painful stimuli or a lowering of the patients' tolerance of them. The mean value of the warning/pain response ratio was significant at the .001 level.

In the second paper Piercy *et al.* (38) applied the same technique to ascertain the difference between the 18 patients psychiatrically assessed as "more anxious" and the 18 assessed as "less anxious" in a group of 36 cases. They found no significant difference for the warning/pain response ratio, but in addition determined the amplitude-ratios for responses to a sudden deep inspiration, to the responses to pain and to warning. In both cases, viz., pain/breath and warning/breath, the ratios for the groups differed significantly at the .01 level. As might be expected, the association of clinically manifest anxiety with anticipatory reaction, and the diminution of the latter *pari passu* with alleviation of the former, is borne out in these experiments. The causal interrelations of these findings clearly require further study.

Turning to more peripheral parts of the pain mechanisms, we may note the observations of Hall (39). Birren, Casperson & Botwinnick (40) and Hardy, Goodell & Wolff (41) found that artificially induced changes in skin temperature bring about changes in pain threshold, a lowering of the latter following a rise of the former. Hall, using the Hardy-Wolff-Goodell technique, measured the threshold in a number of depressive-neurotic subjects and found correlations of $-.83$ ($P < .001$) for males and $-.63$ ($P < .01$) for females with their standing skin temperatures. The place stimulated was on the back of the hand. Hall further observed that in 29 out of 50 cases there was a measurable increase of skin temperature during the experimental session. He suggests that the sequence pain-stimulus \rightarrow cutaneous vasodilation (shown by rise of skin temperature) \rightarrow lowering of pain-threshold constitutes a defense mechanism, the vasomotor reaction being produced by "cortical factors of conditioning and learning which thus mediate" the defense function. It should perhaps be noted that the magnitude of the effect bore no relation to the neurotic symptomatology and that in 9 out of the 50 cases there was a decrease of skin temperature. Here again there is scope for further study of the precise factors involved.

Dorpat & Holmes (42) have investigated the relationship between the pain evoked by continued muscular contraction and that produced by ischemia. These they find to be qualitatively similar. The intensity of the contraction-pain and the subjects' endurance of it are directly related to the strength of the contraction. With strong sustained contractions the onset and magnitude of the pain are unaffected by occlusion of the circulation, whereas with weaker contractions the onset is earlier, pain more intense, and endurance shorter with occluded, than with open, circulation. This suggests to the authors than in contraction-pain the operative factor is an ischemia. They adduce further observations in which tonically contracted muscles are found to be relatively ischemic, as indicated by muscle-temperature, and suggest that in contraction "noxious metabolites" are produced (Lewis' Factor P.), muscle-potassium being one of the components involved.

At the peripheral level, study of the effect of a harmful stimulus in producing enhanced nocceptive sensibility has been continued by Harpman (43) and Harpman & Whitehead (44). By causing small lesions in human skin by chemical agents, crushing, burning, and freezing, they sought evidence as to the neural or humoral mechanism of the "flare" and hyperalgesia studied by Lewis and others. The same effects develop in a sympathectomized limb, suggesting that they are not brought about by excitation of central nervous mechanisms. The fact that the flare and hyperalgesic areas induced by an injury near the mid-line (e.g., of the abdomen) spread across it and beyond the overlap of innervation would also seem definitely to suggest a humoral agent. But in the present investigation the author (43) found that the spread is blocked by infiltration of the skin along the mid-line with procaine, indicating a nervous mechanism at work. Furthermore, if the injury is carried out within a region of skin already anaesthetized, the flare does not develop until the anaesthesia has passed off. This would also seem to indicate dependence of the effects upon neural processes. On the other hand, the author confirms the observation of Lewis that the spread of hyperalgesia from an injury tends to follow the course of veins in the neighborhood and is exacerbated by their congestion. This would seem, again, to point to a humoral agency. In the face of all these observations the author concludes that both a peripheral nervous and a chemical process are involved, their respective roles and interactions being obscure.

Harpman & Whitehead (44) in the second part of this communication attempt to identify the chemical substance involved by injection into the dermis of small quantities of likely substances. Among a considerable selection, which included histamine and decoctions of human muscle and (central) nervous tissue, the only one which produced effects akin to, though not as long-lasting as, the injury itself was an extract of epidermis. The authors attribute the hyperalgesia to the continued production by the injured region of protein breakdown products.

It will be seen that prevailing interest in pain continues much along the lines which have been familiar for some years past. If any outstanding feature is discernible it lies in increasing attention to the physiological reactions which follow pain stimuli, and the part which these reactions in turn may play in determining, qualitatively and quantitatively, the experience invoked. It seems clear that these reactions are complex and probably numerous in type. They range from direct reflex changes in the working condition of the peripheral sense organ to the establishment of stable long-term patterns of reaction in the highest centers. Their relation to other features of individual organization and of psychopathology remains puzzling.

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INDIVIDUAL DIFFERENCES^{1,2,3}

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In the first paper on differential psychology as such, Binet & Henri (10) defined the area as primarily concerned with the determination of "the nature and extent of individual differences." Since that time, progress in the application of individual measurement to practical problems has been remarkable, but concern with basic issues has lagged behind. The study of the "nature and extent of individual differences" begins, as the phrase implies, with the identification of the variables which differ, from individual to individual or from time to time or both. The psychologist cannot write scientific laws without specifying the variables, that is, the traits or the dimensions of individual difference which are related by these laws.

There is nothing new about this argument. Eysenck, for example, refers to . . . the importance of taxonomic work, of finding the dimensions along which we want to measure before we proceed to carry out any measurement. At the present moment, psychology and psychiatry are still in the pre-classificatory stage—although some optimists appear to think that both disciplines have outgrown this stage (40, p. 4).

(Note that as Eysenck uses it, and as we intend it, "taxonomy" denotes an array of psychological dimensions rather than a typology of persons.)

During 1954 to 1956 psychologists have purported to measure such variables as anxiety, intrapunitiveness, stress tolerance, variability, rigidity, sense of personal adequacy, conscientiousness, castration anxiety, personal complexity, identification with Father, humorousness, degree of cortical inhibition, and hypomanic overcompensation, to name but a few. Clearly, there is no limit to the number of dimensions of human difference which could be defined given sufficient ingenuity and patience. Clearly, some of these variables are less significant than others. To the scientist, the useful variables are those which map the domain of his interest so as to allow a nomological network of minimum complexity. We would regard the proper objective of differential psychology to be the identification, with increasing specificity,

¹ The survey of the literature covers the period from May 1954 to April 1956. Much of the 1954 to 1955 material has already been treated by Cronbach in the chapter "Assessment of Individual Differences" in the last Review.

² The following abbreviations are used in this chapter: CFF (critical flicker fusion frequency); GSR (galvanic skin response); IDs (individual differences); MAS (Taylor Manifest Anxiety Scale); MMPI (Minnesota Multiphasic Personality Inventory); S (subject); SES (socioeconomic status); SVIB (Strong Vocational Interest Blank).

³ It is a pleasure to acknowledge our indebtedness to Dr. Kenneth MacCorquodale who generously gave us his skilled editorial assistance. He is not, of course, to be held responsible for the content of this chapter.

and the measurement, with increasing precision, of this kind of variable.

Descriptive psychologists, led by the factor analysts, have evolved a criterion for identifying "this kind of variable." It is believed that the behavior of the individual can be analyzed into clusters of responses which in some sense go together, are functionally equivalent, and imply the operation of some unidimensional, organizing process. It is postulated that among these functional trait unities will be found the variables with which behavior theory, micro or molecular, can construct its laws. One may or may not choose to subscribe to this faith. One must, however, agree with Cattell that "... scientific psychology—as distinct from popular psychological chat—has to pass, as the other sciences such as biology and chemistry have passed, through a phase of concentration on description and measurement" (20, p. 3-4).

As the foregoing makes clear, this review is trait-oriented. The studies reported were selected for their relevance to a trait at issue or for their contribution (positive or negative) to our knowledge of effective methodology.

METHODOLOGY

It seems undesirable to specify a method of choice in differential psychology. When a method is specified, it is commonly factor analysis. In the matter of trait identification, however, factor analysis is but one step in the process and a step for which alternates exist. As the major factor analysts have steadily insisted, the status of a trait, its scientific utility, and its "reality," depend on its identification and measurement in a variety of situations by a variety of techniques.

An excellent example of the contribution that factor analysis can make to trait identification is found in the reports of the symposium on spatial abilities by Fruchter (55), Zimmerman (147), and Michael (96). Fruchter's historical survey stresses the complexity and number of spatial factors described. Zimmerman points to the development of hypotheses concerning the nature of particular factors and describes further factor analyses which were run in an attempt to explicate these hypotheses. Michael presents the kind of methodology which has been widely recommended and so infrequently practiced: a combination of the experimental method with correlational techniques. The fact that Michael's is an outline of a hypothetical experiment suggests the rarity with which this course is actually pursued. It serves to illustrate, however, the status of the factor as a hypothetical construct and calls attention to the fact that like any other construct it needs to have a set of relationships built about it which will enable one to make psychological interpretations of it.

Even in a factor-analytic illustration, it is clear that no single methodology can claim precedence, since trait identification is a general hypothetico-deductive task. In some context of discovery, hypotheses concerning the nature and effects of trait *X* are generated. Confirming evidence is sought in a variety of situational and methodological settings where one's hypotheses

predict observable behaviors relating to the trait construct. Data are collected by the several methodologies to appraise the fate of the hypotheses or more likely, the hypothetical system.

A systematic presentation of this point of view has been made by Cronbach & Meehl (32). This appears to be one of the most important papers for the differential psychologist appearing during the span of this review. We feel that it represents an explicit formulation of the change in the *Zeitgeist* which has been more and more apparent for the last decade. World War II furnished the psychologist with more correlation data on more diffuse tasks and materials than he ever had available before. While much of this was used effectively in performing a host of important practical predictions, two notable disappointments were observed (as Travers (131) has pointed out): first, additional intensive work did not much improve the predictive accuracy obtained, and second (and perhaps causally) there was little or no development of a framework or theory for prediction of achievement or of a theory of differential psychology. This apparent limitation in the "old-line" correlational approach has led many psychologists to diversify their attacks and re-examine their aims and goals.

The nature of this diversification has lacked the formal statement which we feel the Cronbach & Meehl article provides. In briefest possible form, they say that psychology is concerned with many traits and qualities which are not operationally defined and which must be estimated by the use of indirect measures. These are constructs for which no definite criterion measure exists and for which no universe of content-to-be-sampled can be specified.

The general principle behind construct validation is referred to as the "nomological net," an interlocking system of laws which constitutes a theory. At least some of the laws must involve observables. Learning more about the construct is a matter of elaborating the nomological network or increasing the definiteness of the components. This point of view appears to conflict with that discussed by Gaylord & Stunkel (57) and Spiker & McCandless (117) who stress a more "positivistic" attack. They seem to fear that a construct is a refuge for the soft-headed or fuzzy-minded. But the construct must always rest on data; "... unless the network makes contact with observations, and exhibits explicit, public steps of inference, construct validation cannot be claimed" (32, p. 291).

During the period of this review, there have been interesting examples of taxonomic methodologies representing departures from the traditional correlational approach. The first we have labeled "rational factor analysis." Travers (131), for example, attempts to build a theoretical structure for research on aptitudes for training. He discriminates five categories of variables: (a) extent to which subject has already acquired the responses to be learned, (b) extent to which subject has acquired the responses which are prerequisites to learning, (c) learning-set variables, (d) extent to which subject has the abilities to make the discriminations which are necessary to profit from training, and (e) motivation variables. In addition he discusses some of the

possible interrelationships of variables in several of the categories. It should be noted that Travers is not yet talking about traits as we are here but about classes in which are grouped "traits" which may be expected to have common types of functions. Very significantly Travers proposes to try out his program with a series of laboratory studies.

Rokeach (105) chooses a somewhat narrower domain and attempts to give a detailed theoretical statement of the construct of dogmatism. In an abstract, Lewinian-type analysis he invokes concepts like "perceived distance" and "boundaries" in a postulate set which is exemplified in terms of political, religious, and other beliefs. This is clearly an attempt to conceptualize an IDs domain, showing relationships to observable behaviors. Unfortunately it is still programmatic.

In a more advanced fashion (i.e., further along with respect to data applications) Cronbach (30) analyzes the abilities involved in "understanding of others." He shows that the judging operation "... involves many unsuspected sources of variation so that scores are impure and results uninterpretable." He adds, "Studies based on myopic operationism are largely wasted effort when the operation does not correspond to potentially meaningful constructs." His mathematical argument shows that judging others depends on several "attributes" of the judge which may enter into his perception of any individual and cannot therefore be regarded as evidence of his ability to interpret the behavior of another. He shows how IDs may finally be teased out by proper design. The pressing need for such an analysis may be seen by examining Taft's (124) collection of studies which have treated ability to judge others as a unitary personality trait. The studies result in a variety of contradictions, are characterized by low reliability, and seem to reveal a host of specific factors. Cline's (29) extensive study of 316 judges reached a similar conclusion as to the complexity of judging and judge's attributes. Certainly rational analyses are long overdue.

An interesting example of "rational factor analysis" on still another level is found in the work of Schuell (111) in the preparation of a diagnostic-prognostic test battery for aphasics. Aphasia is first considered as related to three cerebral systems: auditory, visual, and sensorimotor, and, second, as related to four language modalities in which impairment may be observed: understanding what is said, talking, reading, and writing. The impact of each of the system injuries on each of the modalities was considered and diagnostic tests constructed (e.g., writing might be defective in different ways depending on which system or combination of systems was involved). The "rational" battery and its subsequent refinements seems to yield five fairly clear patterns of aphasia which have both diagnostic and prognostic significance.

Witkin *et al.* (143) report an excellent experimental attack on a new dimension of IDs: perception of the upright or spatial orientation. Beginning with laboratory phenomena, the investigators discovered the Ss differed in the extent to which they relied on visual as opposed to postural cues. They

showed that IDs were consistent in a variety of perceptual situations [although cf. Gruen (59)]. In this regard the work represents a useful and model study. However, as the investigators go on to seek relations with general personality variables, the study "brings out in bold relief the methodological and theoretical weaknesses of the intemperate 'clinicizing' of behavior," to quote Postman's detailed review (102). Perhaps this study is most valuable as an indicator of the contributions that can be achieved by careful laboratory study and as a warning of some of the perils that can beset investigators who attempt to collect personality correlates.

Our methodological examples might well end with a *tour-de-force* from the experimental side. Zeaman & Kaufman (146) took a simple, well-studied motor learning task and posed for themselves the problem of accounting for IDs in terms of Hull's constructs. They discovered from his equation that differences in amplitude of response might be attributable to any of 11 variables. Assuming variation in each of the 11 parameters in turn, they generated predictions of change in amplitude over time. Six different curve patterns emerged. The first experiment clearly showed one of the predicted patterns and narrowed the field down to a choice between two of the Hull variables. These variables permitted differential predictions in two additional experiments, whose results provided the information needed to choose between these two constructs. We think both experimental and differential psychologists may profit from this unusual example of identifying a variable, and we feel that we all may take cheer in the finding, "... although the top-heavy theoretical superstructure of Hull's system makes its use cumbersome and unwieldy in this area, it nevertheless can be deductively elaborated to yield testable predictions of a semi-quantitative nature in the field of individual differences" (146).

FACTOR ANALYTIC STUDIES

While factor analysis is still the most popular method of attempting to discover trait unities, the present writers find it less and less impressive as a "search" technique and distinctly disappointing in its applied or theoretical successes. As Cronbach (31) pointed out, it has made limited contributions to applied problems. In the theoretical field we are recognizing, as Lazarsfeld pointed out (84, p. 12), "that factor analysis was really a special case of model building; the computation of factor loadings was a special case of identification of model parameters. . . ." Factor analysis has no more claim on the differential psychologist than any other form of generating hypotheses from a matrix of data. If there are good reasons to believe that the linear additive model does not apply to the situation involved, then factor analysis is indeed contra-indicated [cf. Guttman (64); Gulliksen & Tucker (63)].

These considerations are mentioned in order to combat the view that the possibility of an adequate taxonomy of psychological variables stands or falls with the success of factor analytic researches. These researches have now been many in number and large in scope and, on such a view, their achieve-

ments to date might well be grounds for considerable pessimism about the future of scientific psychology.

In the factorial study of intellectual functioning, Guilford's laboratory continues to dominate the scene. Their study (62) of some 55 reasoning tests on a large sample of Air Force men resulted in the extraction of 16 factors, of which 7 had turned up in three prior studies. The authors conclude that 5 reasoning factors have been well established. Guilford (60) in discussing some of the many interpretations which have been given the general reasoning factor during the 15 years since it was first recognized, concludes that, since "each of the great variety of problems calls upon its own pattern of abilities for solution," the "general reasoning factor is probably limited to certain types of problems that have something in common with arithmetic-reasoning problems."

A study by Wilson *et al.* (142) of creative-thinking abilities in a similar population uncovered 14 "identifiable" factors; 9 of these had a familiar look and 5 were new. Hertzka *et al.* (69) studied evaluative abilities in air cadets, again identified 14 factors of which 6 were old friends and 5 or 6 others appeared to relate to evaluative ability. Matin & Adkins (91) did a second-order analysis of the 13 reasoning factors found in an earlier study, getting good simple structure for the 6 factors found, 5 of which were interpreted.

Guilford *et al.* (61), in their study of interests, provide an illustration of an approach one step removed from the strictly "blind" factor analysis of a behavior domain. Extensive preliminary hypothesizing about the nature of primary interests and their manifestations led to the development of a 1,000 item battery designed to cover comprehensively the area as thus rationally analyzed. The battery was given to 600 airmen and 720 officer candidates and factored separately for the two samples. Seventeen factors appear in both analyses, 28 interpretable factors in all, of which 6 or 7 are directed toward vocational stereotypes, and some 19 may be regarded as "basic interest factors."

Fleishman has attempted to integrate factor analytic findings relative to psychomotor ability. In one study (48), 38 tests of both apparatus and printed type were designed to represent together in one matrix nearly all of the psychomotor factors previously identified. The complete battery was given to 400 airmen. A centroid analysis and "blind" rotation gave 12 factors, including apparently at least 6 of the 7 factors originally hypothesized. The previously defined factor of psychomotor coordination was not broken down in this analysis, although Fleishman & Hempel (50) subsequently reduced this factor to more elementary components.

A good, careful study of its type, this study illustrates again that one can always expect a core of familiar factors to turn up repeatedly but that one must also expect at least some old ones to be missing and at least some new patterns to appear in each new factorization. In the area of psychomotor abilities, it appears that some tests have been established as relatively pure indicants of a single factor but that their loadings tend not to be very high.

In many of the tests used here, for example, over half the variance was shown to be specific. It is somewhat unsettling to note that even Fleishman, who knows this domain as well as anyone, cannot always get the tests to sample the right factors, e.g., his "aiming" tests do not appear on the "aiming" factor.

Hempel & Fleishman (68) analyzed a battery of 46 tests including 17 manipulative apparatus tests, 6 printed tests, and 23 gross physical performance tests. Again the sample included 400 trainee airmen. Fifteen interpretable factors were extracted, a principal finding being that no factor overlapped the areas of fine manipulative and gross physical proficiency.

In another study, Fleishman & Hempel (49) tested the generality of earlier findings concerning the changes in the pattern of abilities contributing to performance on a task as practice increases. Previous work with a complex psychomotor task indicated that nonmotor factors accounted for most of the predictable variance during early trials but that motor factors became increasingly important with practice. In the present study of 264 trainee airmen, the S's received 16 trials on a paced-visual discrimination-reaction task in one continuous session. Factor analysis at successive stages of acquisition indicates as before that there are systematic, progressive changes in factorial structure with practice. In the early stages, spatial relations, verbal comprehension, and psychomotor coordination are the chief contributors whereas later on such factors as rate of arm movement, reaction time, spatial relations, and perceptual speed begin to dominate.

Rim (104) provides a neat analysis of the dimensionality of perseveration and fluency and their relation to Eysenck's introversion-extroversion dimension. He collected from the literature 21 tests of perseveration and 6 tests of oral verbal fluency and administered this battery to 121 S's sampling 5 psychiatric categories and a control group of 25 normals. Neither the perseveration nor the fluency tests distinguished the neurotics from each other or from the normals. A separate analysis of fluency showed that two factors were required, one defined by the three tests in which relatively little restriction was placed on the responses ("Name as many towns as you can think of") and the second defined by three tests involving greater restriction ("Name as many words beginning with 'a' as you can think of"). Four factors described the perseveration matrix, including the "classical" disposition-rigidity or perseveration factor. The others are rather difficult to interpret but appear to refer to separate aspects of the perseveration notion. The author concludes that fluency and perseveration are not unitary traits and that neither is related to the extroversion-introversion dimension inasmuch as none of the factors found separated the hysterics from the anxiety neurotics in his sample.

Keehn (80) inquires into the dimensionality of color-form dominance in perception. Nine color-form tests were administered to 200 normal S's in speeded form and the tetrachoric intercorrelations factor analyzed. Two factors were extracted, the first interpreted as one of color-form attitude and

the second being somewhat obscure but appearing to separate the pure grouping tests from those requiring an element of memory. However, the average intercorrelation was only .38, indicating low saturation of the several tests with the color-form factor.

The large-scale, long-term program of factor analytic studies of personality by Cattell and his colleagues has been coming to fruition during 1954 to 1956. Separate studies, employing large numbers of measures and large samples, have analyzed rating, questionnaire, and objective test data on adults and on children. Cattell & Gruen (25) repeated the adult questionnaire study on a sample of 330 children between the ages of 9 and 12. Fourteen factors were obtained, four psychologists being able to agree as to the meaning of 10 of these. A separate study of 70 children yielded odd-even reliability estimates for these factors. Four of the five factors with the highest variance and with reliabilities above .59 were identified with factors in the adult study, viz., emotional sensitivity, nervous tension, emotional immaturity or anxiety-depression, and will control or character integration.

The genetics of these factors and others was the subject of a large scale twin study by Cattell, Blewett & Beloff (23). The sample included 104 monozygotic and 64 dizygotic twins, 182 siblings reared together, 72 unrelated children reared together, and 540 unrelated children. Of the 12 factors represented in the Junior Personality Quiz, those showing an appreciable influence of heredity included energetic conformity, socialized morale, dominance, and impatient dominance, while those indicated to be mainly genetically determined were cyclothymia versus schizothymia, adventurous cyclothymia versus submissiveness, and intelligence. By way of comparison, Vandenberg (137) reports preliminary results of the Michigan Twin Study based on a sample of 45 pairs of identical and 37 pairs of like-sexed fraternal twins of high school age. Values of h^2 for Cattell's Junior Personality Quiz factors range from .47 to .69, as compared with .44 for Raven's matrices, .71 for finger dexterity, and .71 and .61 for left and right hand card sorting, respectively.

Completing the systematic survey mentioned above of the 11 year age group is the analysis by Cattell & Gruen (26) based on objective tests. Wherever possible, the same or similar tests as had been earlier employed with adults, 128 tests altogether, were administered to 200 boys and girls in the age range from 9 to 12. Cattell & Gruen feel that the personality structures found in children are largely the same as in adults. "There are, however, indications of modification of loading emphasis." A 12-factor objective test battery has been developed for group administration to adolescents and is being employed in the continuing nature-nurture study mentioned above.

In 1955, Cattell (21) reviewed "the chief invariant psychological and psycho-physiological functional unities found by *P*-technique." He found only five comparable studies employing the correlation over time of psychological and physiological variables. Seven factor patterns are identified as being "reasonably invariant from study to study" and five others "are suffi-

ciently statistically substantial to justify some statement and hypothesis formation." These 12 factors are largely defined by physiological measures and seven of them are considered to be matchable with factors previously found by *R*-technique. Cattell remarks that whereas factors which are stable over time would not appear in *P*-technique, "the converse proposition, that some *P*-factors will fail to appear as *R*-factors, should not hold." This last does not appear to us to be correct. Consider, for example, two physiological variables, *A* and *B*, which vary perfectly together over time but whose difference in measured units, *A-B*, is an independent variate distributed normally across people. The *R*-correlation between two such variables could well be zero in spite of perfect *P*-covariation.

Cattell, Dubin & Saunders (24), in an attempt to verify factors hypothesized on the basis of previous work, studied 115 objective personality tests on a sample of 100 male students. They interpret 17 factors of which 7 or 8 are felt to be well enough confirmed to justify individual intensive study to find more valid, highly-loaded measures."

In another paper, Cattell reviewed "the principal replicated factors discovered in objective personality tests" (22). Cross matching of some 150 such factors, most of them turned up in studies from his own laboratory, leads him to identify 18 "potentially invariant patterns" of which he considers 12 to be satisfactorily invariant and established. The art of factor matching is discussed at some length in this article. The new "salient variables similarity index" is described and consideration is given to the various criteria which determine the expert's final judgments of factor similarity or identity.

Here, Cattell reiterates his own view of the function of factor analytic exploration in the taxonomic enterprise.

The patterns presented here may be regarded in themselves as hypotheses to be tested—hypotheses concerning functional unities or lawful recurrences of form. In investigating these hypotheses with the aim of proceeding to a precise check on each pattern and to further interpretation, through discovering more highly loaded variables than any yet known, psychologists may now concentrate on single factors and therefore on lengthened tests requiring more time-consuming experimental procedures. . . . With the definition of these factors and the development of more saturated measures for them, the way should be open for testing hypotheses about their nature by controlled experiment and by correlation with everyday criteria.

Lorr *et al.* (86) did a factorial study of behavior ratings of hospitalized psychotics. Of 11 oblique first order factors extracted, only 2 could be clearly identified with factors found in other studies; 5 more were considered possible matches. Three second-order factors were obtained in this study. Lorr & Rubinstein (87) present a similar analysis based on a sample of 184 veterans receiving outpatient psychotherapy. Fifty-eight rating scales were intercorrelated, yielding 10 identifiable first-order factors, of which 3 were considered to be similar to ones in the Cattell list. Two second-order factors were called "underlying hostility" and "undercontrol vs. overcontrol, overconformity."

When an investigator addresses himself to the factorial study of so broad a domain as "personality," it is difficult from his findings to assess the adequacy of the technique for his purpose. A recent study by Holtzman & Bitterman (74), however, had a more modest objective and the limitations of the findings, therefore, are rather more clearly discouraging. The purpose here was to study the factorial structure of adjustment to stress, based on an extraordinary total of 101 different variables involving "ratings of adjustment, by self, peers, and instructors, objective and projective personality tests, stress tests, a galvanic conditioning situation, perceptual tests, and a urine analysis." The more promising of these measures were selected for intercorrelation (in the case of the 22 GSR variables, a separate factor analysis was employed to reduce this number to 6) so that 41 scores were represented in the final factor matrix. Of the 5 factors identified, the clearest was defined by the ratings of officer aptitude. The second is defined by loadings between .41 and .61 on four tests of perceptual and perceptual-motor functioning; its significance is "unclear." Factor III is defined by the McKinney Reporting Test; Factor V by the MMPI; and Factor IV by the GSR variables. In short, we have here a large scale, military damn-the-expense type of study of an extremely diversified set of variables expected to be associated with reaction to stress. If we are to accept as conclusive the factor structure of the product moment intercorrelation matrix, these variables are not in fact interrelated. Where do we go from here?

In looking over the factor analysis yield of the past two years, we find ourselves with mixed feelings. We are impressed with the great energy which has been expended. We are filled with admiration for a few studies which are either continuing long-term programs or which grew up from very thoughtful research plans. And finally we are appalled by what in some cases seems to be sheer waste of time and money.

But in all the studies, good and poor, one looks in vain for the next step. Where are the superior scales of factorial purity? Where are the traits manipulated, investigated, and observed in nonfactorial settings? Where has the analysis been carried beyond the hypothesis stage? It is our feeling that the "pure" factor analyst can with some justice claim that he is not responsible for the entire job; that he is pointing the way for others. But no one else seems to be doing anything with these promissory notes and the entire enterprise is suffering in consequence.

TRAITS

Intelligence.—Certainly the most venerable construct (or set of constructs) in the field of differential psychology is intelligence. Two major theoretical contributions are the Cronbach & Meehl article (see above) which bears directly on this issue and the Spiker & McCandless analysis (117) which takes an extreme and, in our opinion, outmoded, operational position, concluding that every new set of items is a new definition of intelligence having precisely the same formal status as any other definition but perhaps having more or less "fruitfulness" or "significance."

It is obvious that their approach is in direct opposition to the Cronbach & Meehl thesis and the position of this review. Spiker & McCandless seem to permit no gradual evolution of constructs. It is our feeling that the Cronbach & Meehl approach will be more useful in making clear what different psychologists are saying about intelligence by stressing where they disagree concerning the hypothesized nomological net of the construct than will the rigid operational approach. Feigl's warning in this regard is especially useful,

Unfortunately, . . . positivism has only too frequently become a doctrine of arbitrary, sterile constriction. In other words it has tended to become a negativism or prohibitionism . . . Comte, Mach, Ostwald, Pearson and many others among the earlier and later positivists and operationists have at times thrown out the baby of fruitful hypothetical constructs with the bathwater of transcendental speculations. In the name of a misunderstood principle of parsimony they have relegated to the limbo of metaphysics concepts and hypotheses which were perfectly good physics, biology or psychology. (46, p. 200)

The utility of the construct validity approach may be illustrated by considering some studies of intelligence. Hess, for example (72), contends that differential performance of high and low socioeconomic groups on intelligence tests arises from cultural factors within the testing instruments and testing situation rather than from genetic or environmental differences in "intelligence." He is apparently contending that the intelligence-defining correlations of two tests could be essentially the same except for their socioeconomic correlation. The question then resolves itself to a matter of fact: Can a test be built that has most of the usual correlates (how many and which ones are not specified) but specifically not the socioeconomic ones? Proposed items were screened by sociologists, psychologists, and cultural anthropologists to eliminate those which were sensitive to socioeconomic differences in vocabulary, material, and motivation. Sixteen items resulted which were given to 545 school children between six and one-half and nine and one-half years of age, divided into three groups: high SES white, low SES white and low SES Negroes. Means for the white groups were not different. The high SES white group was significantly higher than the Negro groups, however. Scores were related to "traditional" intelligence tests and to reading achievement scores within each of the 12 grade-SES subgroups. Experimental and traditional test scores had a median correlation of .43. Correlations with reading achievement are about the same for the two tests for the white groups but are appreciably lower for the experimental form for the Negro groups. Hess emphasizes that this reveals cultural influence in intelligence tests and suggests that standard tests exaggerate socioeconomic differences. We feel that the evidence contributes only slightly toward the construct that Hess is concerned with but that it represents a start at construct validation.

A variant of the reading achievement finding occurs in a study by Justman & Aronow (77), who tested 246 Ss from nine sixth grade classes which were heavily weighted with retarded readers. It was felt that the Pintner Intelligence Test must markedly underrate these students because of its

reading difficulty, but that the Davis-Eells Games would reveal relatively normal scores for these children. Contrary to expectation less than one point difference in mean IQ was found between the two tests. The investigators conclude that "the Davis-Eells Games cannot be looked upon as a markedly better measure of intelligence of poor readers. . . ." This appears to be a case in which agreement with an established test is looked upon as not being part of the net belonging to the construct "good intelligence test."

A final study in this subgroup is that of Haggard (65) who studied test-retest scores of 671 11-year-old children, selected from upper and lower SES groups, on a test of 40 items "taken from standard intelligence tests." He varied practice, conditions of practice, form of the retest (half had items modified to "eliminate middle class bias"), and conditions of the retest. Practice helped both groups equally. Haggard seems to argue that since the gains (not the scores) were equal, this shows that the groups did not differ in "real" ability. (Was this specified in the net?) One group getting only the revised test did better than the groups getting the standard test for the first time. Of these, the high status subgroup showed the most advantage over their counterparts taking the standard test. It is concluded "The mere revision of the test items was not sufficient to reduce the difference in performance between the high-status and low-status children. The marked discrepancy . . . was only decreased when the conditions of motivation and practice were also present."

It is our opinion that all of these studies would benefit greatly by a careful rethinking of just what the nomological net is supposed to look like and just what kinds of data would constitute evidence for and against the constructs that are being proposed.

Terman has presented two major reports of his work. In the Bingham lecture (128) he reviews the development of his own interest in the area of intelligence and the origin and history of the study of gifted children. He raises two questions of special relevance to this review: Can we make better predictions by using factorial data? Can we agree on what the factors are and how they are organized? In a detailed monograph (127) he reports on scientists and nonscientists in a group of 800 gifted men. Data are presented both at the child and adult level on intelligence test scores, vocational interest test scores, biographical data, parent ratings, parent-child relationships, etc. He feels that the study yields many clues as to the differences between scientists and nonscientists although the biographical data were in general disappointing as were the "motivational" items. He suggests how further specific research might be done on the determiners of science and nonscience careers in gifted children.

In a similar vein, Bayley (4) in her presidential address gives a valuable summary of the Berkeley Growth Study data on the growth of intelligence. The familiar finding that no behavior before age two to three predicts later intelligence test scores is amplified by Hofstaetter's (73) analysis of the data showing the predominant factors to be "Sensory-motor Alertness" up to two

years, "Persistence" from two to four years and "Manipulation of symbols" ("g"?) from 4 years to 18 years (which was as far as the data went). Bayley attempts a Thurstone-type growth curve for descriptive purposes and reports that the scores continued to increase until at least 21 years of age. There were differences in rate of approximating parents' relative level, and in growth rates. Adult intelligence appears to continue to grow (at least for these selected high ability samples) and Bayley & Oden (5) report that Concept Mastery scores increased significantly for Terman's gifted group on a 12-year test-retest at maturity (from average age 29 to average age 41).

Owens (98) continues his report on a 30-year test-retest of 127 male adults on the Army Alpha. The test-retest reliabilities ranged from .30 for "Following directions" to .69 for "Arithmetic reasoning." Total scores correlate .77, revealing, as Owen says, "remarkable consistency."

Several relevant monographs discuss the intelligence construct. Bischof (11) relates a short, simple history of the statistical theories of intelligence and gives samples of several kinds of test batteries. Fromm & Hartman (54) present an extended discursive analysis making the realistic point that neither intelligent behavior nor intelligence test scores are independent of emotional behavior and experience.

In a similar fashion, Heim (67) devotes a full book to the clinician's point of view concerning intelligence tests. The argument is largely an arm-chair analysis supported by observation and general good judgment but little data.

Bray (16) has written a monograph which attempts to develop a framework for the study of talent and superior performance. He considers general intelligence, discusses evidence from factor analysis, looks at nonintellectual variables such as motivation and work performance, and concludes with a design for a study of "talent" and "superior performance." In our opinion his framework rests on several shaky assumptions which are much like those of the Chicago group and are as yet poorly confirmed as adequate working bases. These assumptions seem to be more and more commonly accepted and thus deserve treatment here. The first assumption is that people differ innately in intellectual potential (indeed, we are told that genetic determiners should be accorded three or four times the "weight" of environment). The second assumption is that the average innate capacity of children in one socioeconomic-cultural group does not differ substantially from the capacity of children in another group. Therefore, the wide differences in IQ among children from the different groups must be attributable primarily to differences in cultural opportunity. The "hereditarian" wins the battle and loses the war! The reviewers do not understand why the first and second assumptions are seen to be so compatible and do not for that matter see why the second is deemed plausible at all. Surely if the major part of intelligence is genetically determined and if there is good vertical social mobility in the culture, then it will become very likely that on the average some differences will exist between the native endowments of children whose parents have

been differentially selected on this variable. We find ourselves, therefore, very sceptical of the conclusion that population intelligence potential can be estimated by observing the most-favored cultural group and thus we are out of sympathy with the rest of the book. Factor analytic results are regarded almost solely as cultural patterns, and the final design is an interview attack designed to concentrate on those events in the history of the talented subjects which seem to have influenced the superior performer and not his mediocre control. We feel that Terman's suggestions might be more fruitful and that the after-the-fact nature of Bray's design will greatly weaken its contributions to our knowledge.

Anxiety.—Anxiety is easily the most fashionable psychological variable at the present time although hotly pursued by "authoritarianism" and "rigidity." Since the importance of anxiety has been understood by psychologists at least since Freud (and by the ordinary man since a good deal earlier), the cynic might be tempted to ascribe the recent rash of studies of this phenomenon to the development a few years ago of a test which is easy to give to large groups of subjects and which purports to measure "manifest anxiety."

Implicit in a number of the studies to be mentioned, and explicit in one or two, is the notion that anxiety, or at least one kind or dimension of anxiety, can be fruitfully studied by a construct validity approach. By amassing a large number of correlations between the MAS and other variables, something is to be learned about both the construct in question and about the MAS. [We cannot, however, condone the violence done to the construct validity idea by one author who argues that, since the MAS predicts poorly such criteria as careful anxiety ratings, but correlates consistently low positive with a host of other variables, it may be said to have "low criterion validity but good construct validity." (113)!] According to the Technical Recommendations (148),

in studies of construct validity we are validating the theory underlying the test. The validation procedure involves two steps. First the investigator inquires: From this theory, what predictions would we make regarding the variation of scores from person to person or occasion to occasion? Second he gathers data to confirm these predictions.

The theory underlying the MAS has never been clearly enunciated. The test was originally constructed by asking clinicians to select from a pool of standard personality questionnaire items those items which would likely be endorsed by persons suffering from high "manifest" anxiety. Some of the postulates which would appear to be involved, therefore, in the "theory" of this test include: (a) that there is some unitary state (or response), with respect to whose intensity people vary, which may reasonably be called "manifest anxiety"; (b) that the clinician-judges had reasonably isomorphic notions of the nature of this variable; (c) that the clinician-judges were able to predict validly the normal questionnaire behavior of persons high on this variable (which, in turn, implies the assumption that the clinicians were able

to assess validly the intensity of this variable from clinical observation of individuals): and (d) that the total number of such items which an individual testee endorses approximates to some monotonic increasing function of the intensity of the variable in that subject at that time, over the full range of such intensity observable in the general population.

Let us now consider some of the findings relevant to the MAS which appeared during the period covered by this review. Hoyt & Magoon (75) asked 8 counselors in a student counseling center to rate 289 counselees as having high, medium, or low anxiety. The mean score of the high group was 20.6, of the low group 11.7, this difference reaching significance. Predicting the high-medium-low trichotomy from MAS score gave about 52 per cent misclassification where chance expectancy would be about 67 per cent. Buss (18) in a similar study on psychiatric patients found an r of .60 between pooled anxiety ratings by four psychologists and the patient's MAS score. Buss *et al.* (19) report that by carefully specifying observable symptoms as criteria, their clinicians were able to achieve an average interjudge correlation of .83 in rating manifest anxiety in patients from interviews.

Taylor & Spence (126) were unable to differentiate anxiety neurotics from other neurotics with the MAS nor did these groups differ significantly in eyeblink conditioning scores or psychiatrist's ratings of anxiety. A group of psychotic Ss, mostly schizophrenics, conditioned significantly faster than the neurotic sample but did not show higher MAS scores or anxiety ratings. Sampson & Bindra (107) find that, in spite of considerable overlap, the MAS significantly differentiates neurotics from college students but that the mean score of anxiety neurotics does not differ from that of other neurotics. They find that the anxiety neurotics tended to score in the range from 19 to 33 (on the 50-item form of the MAS) whereas the other neurotics tended to score above or below this range. They interpret this to indicate that the MAS bears a curvilinear relationship to anxiety [in contradiction of postulate (c) above], and they employ this conclusion in an attempt to reconcile the conflicting findings of Taylor & Spence, Bitterman & Holtzman (12), and others. However, Matarazzo *et al.* (89) failed to confirm this relationship, finding no systematic distribution of the scores of 85 neuropsychiatric patients with respect to diagnosis. They find, however, a significantly higher mean score for psychiatric than for nonpsychiatric patients, both for inpatient and outpatient samples, the extent of the separation being indicated by a point biserial r of .58. Finally, Siegman (113) reports that anxiety neurotics do get significantly higher MAS scores than do other neurotics or than schizophrenics, and that psychopaths score lower than any of these.

The original studies by the Iowa group indicated a tendency for high-scorers on the MAS to show faster eyeblink conditioning than low-scorers, a result interpreted to be attributable to the higher drive level of the "anxious" group. Similar results were reported during 1954 by Spence & Beecroft (115) and by Spence, Farber & Taylor (116). However, the actual differences reported have in all cases been small and where the covariation between MAS

scores and a measure of conditioning has been calculated, the value has been low positive or insignificant [e.g., Franks (52); Bitterman & Holtzman (12)]. Bindra *et al.* (9) compared Ss scoring high and low on the MAS on a measure of salivary conditioning and found no differences, a result which leads them to suggest that "anxious" Ss under the stress of the experiment exhibit greater muscle tension and a reduced threshold for the eyeblink but not for the salivary response.

Parenthetically, the dimensionality of "conditionability" is of considerable interest. Certainly there are IDs in the speed of conditioning, in the size of the increment contributed by each reinforced trial, or in the height of the asymptote of the conditioning curve or both. Will the number which describes Jones' eyeblink conditionability also specify, *ceteris paribus*, his relative facility for acquiring a conditioned GSR? Such considerations are of particular importance to an investigator like Franks (52) who attempted to test the hypothesis that introverts are more "conditionable" than extroverts. The test-retest correlation (2 to 3 week interval) for his conditioning measures was .52 and .40 for eyeblink and GSR, respectively, and the correlation between the two measures even when corrected for the considerable attenuation is only .52.

The primary finding of the Franks study was that a group of anxiety neurotics, the prototypic introverts, show greater conditionability than normals whereas a group of hysterics, the extreme extroverts according to this system, show less. But when Franks plots the acquisition curves for these groups it is apparent that they were not equal in the strength of this response on the very first trial, i.e., before any conditioning could have taken place, and that these original differences have not appreciably altered by the end of the conditioning series. Conditionability presumably would be properly measured by the slopes of the acquisition curves. "Total number of CRs elicited," which is the measure used by Franks (and others), can be grossly contaminated by any differences there may be in the original level of the response (as can the "number of trials to criterion" measure). Since the slopes of his group curves do not appear to differ, we cannot agree that Franks has shown that "anxiety states conditioned much better than hysterics." Moreover, in view of the negligible correlation between his two conditioning measures and the uncertain dimensionality of the "conditionability" trait, we consider it especially premature of him to conclude the "conditionability is related to introversion-extroversion."

Returning to the studies of the MAS, we find Hughes *et al.* (76) failing to confirm an earlier finding of Taylor & Spence that high and low "anxiety" groups differ in ability to learn verbal response alternations. Wenar (138) found faster reaction times in high-scoring Ss but failed to demonstrate the expected greater effect on this group of increasing stimulus (shock) intensity. Kamin (78) gave 70 Ss a discrimination task involving avoidance of shock and found no correlation between performance on this task and MAS scores. However, a test of mechanical aptitude and information correlated

positively with discrimination performance and negatively with the MAS. Kamin suggests that persons with high MAS scores may be more stressed by experiments involving fancy instrumentation because of their low aptitudes for, and familiarity with, mechanical apparatus and that this difference may account for some of the published findings. In a rather similar vein, Grice (58) finds that low-scoring Ss on MAS do better on a complex discrimination task but that they also score higher on a test of clerical aptitude. Partialing out clerical aptitude reduced the differences on the discrimination task to zero. It is difficult to say at this point whether mechanical and clerical aptitude account for MAS variance or whether "anxiety" accounts for part of the variance of these aptitude tests.

Kamin *et al.* (79) found a correlation of .40 between MAS score and number of false or anticipatory reactions in a discrimination problem involving stress. This same disruption measure correlated $-.36$ with an index of pain tolerance which in turn was not significantly related to MAS scores. The authors conclude that disruption of performance under stress involves at least two independent factors, general emotional arousal, as indicated by the MAS, and avoidance tendency, as determined by pain tolerance. Travers *et al.* (132) illustrate the complexity of the relationship between MAS score and experimentally induced anxiety as joint determiners of performance. On a particular problem solving task, low MAS Ss performed better under nonthreatening instructions whereas moderately high-scoring Ss did much better under threat conditions. Cubic equations described the relationship between performance and MAS score and the curves for the threatened and nonthreatened groups were approximate mirror images of each other. To further complicate matters, when the study was repeated (at another Air Force base) the threat and nonthreat curves exchanged shapes. Similarly, Sarason (108) finds a significant interaction effect between strength of motivating instructions and MAS score on serial learning. Being told that their performance would be a measure of their intelligence detrimentally affected Ss scoring "high" on the MAS but was facilitative to Ss scoring "medium" and, less so, to those scoring "low."

Taylor (125) reviews various findings concerning the relation between MAS scores and intelligence, concluding that the correlation is near zero among college students but that a greater relationship may exist in a less homogeneous population. Matarazzo & Matarazzo (90) find no significant correlation (epsilon) between the MAS and Wechsler scores on a sample of 80 psychiatric inpatients, nor did the MAS show any relationship to a measure of pursuit rotor learning. Smith *et al.* (114) find a significant correlation of .17 between the MAS and number of food aversions. Fattu *et al.* (45) report a correlation of $-.35$ with problem solving ability. Kerrick (82), using a sample of Air Force trainees, found low negative correlations between the MAS and several aptitude and intelligence measures and a correlation of $-.73$ with the K scale of the MMPI. A factor analysis of the MAS interitem tetrachorics (97) suggests generally low communalities and no less than five

rather weak factors running through the items. Eriksen & Davids (38) reports rho's of .41 and $-.87$ between the MAS and careful ratings of repression tendency and optimism, respectively. Most important of all, these authors confirm what has been a repeated finding [cf. Brackbill & Little (15)], that the MAS and the Pt scale of the MMPI correlate to the limit of their reliabilities, .92 in this case. [Franks (52) found a correlation of .92 between the MAS and the Maudsley Medical Questionnaire, a neuroticism measure essentially identical to the Pt scale.]

It appears, then, that the MAS measures a variable which Eysenck calls "neuroticism" and which we might describe as the tendency to characterize one's self as sick, maladjusted, having symptoms. Eriksen (36) finds that normals scoring high on the Pt-MAS variable tend not to show the defensive forgetting phenomenon, apparently because such people characteristically deal with threat by being hyperalert to it, by assimilation, and by intellectualization rather than repression. The MAS seems to measure neither anxiety in the ordinary sense of momentary arousal due to fearful apprehension nor in the sense of long-term anxiety proneness, persisting tension, low threshold of alarm. More nearly, it "measures" the Ss tendency to react to anxiety with the mechanisms which, in their extreme form, are characteristic of the psychasthenic neurotic.

It would appear that the "theory" upon which this test is based cannot be said to be confirmed. If the clinicians who selected the items originally were able to predict how anxious people would respond, then certainly their ratings of anxious people should correspond with the MAS scores. But other clinicians, in a variety of settings, cannot achieve a greater correspondence than that represented by an r of .60, and the scale cannot even consistently identify anxiety neurotics whose distinguishing symptom is acute, uncontrolled, and certainly "manifest" anxiety. And what have we learned about the construct of anxiety itself in all this? We still know nothing of the dimensionality of this construct, of how many separate variables will be required to describe the relevant phenomena in this important area. We still know no more than we did about the topography of the anxiety response or responses or how to measure it or them (unless desperation is to lead us to persevere in the misguided empiricism of defining "manifest anxiety" to be what the MAS measures).

Thus we come to the conclusion which has been the aim of this extended critique. We regard it as an unhappy misapplication of the important notion of construct validity to seize upon some new instrument which claims, with negligible justification, to measure an ill-defined but important variable and then to proliferate its correlations with other things in the wan hope of somehow discovering something about the variable in question in this way. The MAS is not in any useful way related to any theory about anxiety and therefore a knowledge of its correlates cannot teach us anything about anxiety.

Stress tolerance.—We have already mentioned above the failure by Holtz-

man & Bitterman (74) in a large scale factor analytic study to find by that technique any clear indication of the processes involved in the reaction to stress. Stopol (119) studied college students doing a digit-symbol substitution task under distraction-stress involving loud noises, etc., and again under failure-stress involving humiliation and aggressive comments by the experimenter. Performance declined under both stress conditions, but the degree of decrement (or its inverse, stress tolerance) was specific to the particular stress situation and did not correlate between the two conditions. A question raised by this study is whether or not the Ss were similarly aroused by the two manipulations. Parsons *et al.* (100) attempted to control for degree of disturbance in a study of steadiness of motor control. Again the two stress conditions were distraction and failure. The index of stress was the change in eosinophil count based on blood samples drawn 15 min. before and 15 min. after the stress procedure. It was found that individuals were not consistent in the degree of upset shown in response to the two procedures. The correlation between stress tolerance scores also was negligible, even when corrected for amount of eosinophil change. It is significant that the correlation between the prestress steadiness scores for the two testings was quite low, indicating that the base line against which the other measurements were made was too fluctuant for any real consistency to be expected. These pioneering efforts demonstrate that useful research in this area will have to begin with careful, repeated measurements of each of the phenomena so that we will know under what conditions we can confidently speak of "Jones' characteristic reaction in terms of measure A to stress condition B" [see also Ross *et al.* (106)].

Evidence is accumulating that there are complex interaction effects between degree of stress, the particular manifestations of it (e.g., striate muscle vs. autonomic), and, indeed, perception by the S of the stress situation itself. Eriksen & Wechsler (39) studied discrimination performance in two groups of Ss, one of them made anxious by random electric shocks. It was concluded that the effect of anxiety here was upon response thresholds. In another paper, Eriksen (37) studied stimulus generalization in college students scoring high or low on the psychasthenia-hysteria axis (as defined by the Hy and Pt scales of the MMPI). Again anxiety was induced by electric shock, and it was found that the "hysterical" group showed greater generalization than the "psychasthenic" group. The interpretation given is that the experimental conditions deprived the Hy Ss of their preferred mechanism of avoidance, leaving them relatively more disturbed than the other group, stimulus generalization being considered to increase with degree of uncontrolled anxiety.

Basowitz *et al.* (3) studied soldiers under the anxiety-evoking conditions of paratrooper training, using repeated administration of a set of psychological and biochemical measures thought to be related to the effects of stress. These authors were particularly interested in hippuric acid secretion (an indicant of the ability of the liver to mobilize glycine) which they felt had

been shown by previous work to be a measure of "free anxiety." The results of this elaborate study, however, pose many more problems than they solve. Although it would seem that falling from a variety of high places should constitute a fairly primitive and simple stress situation, it appears that even so homogeneous a group as these paratrooper trainees varied widely in their perception of this situation and, therefore, in their reaction to it. The authors felt it necessary to distinguish between anxiety related to physical danger and anxiety concerned with failure and even suggest that there may be qualitative differences in psychosomatic manifestations. There were wide individual differences both in degree of physiological reaction and in the particular pattern of this reaction, and physiological upset was far from perfectly correlated with psychological deficit or with self-report. Finally, it appears that extreme and long continued stresses as produced by war-time experience or psychiatric disorder involve different psychophysiological correlates than those mobilized by more transient stress conditions in the relatively normal individual.

Adjustment.—A rather strong indication that psychological adjustment does not define a trait unity, and will therefore likely not prove to be a theoretically useful dimension, is provided in a study by Tindall (130). To a sample of 66 teen-age boys in a children's home, this investigator applied 16 measures of "adjustment," including personality inventories, ratings by teachers, psychologists, and peers, an incomplete sentences test, a Rorschach check list, and an index based on a time sample of behavior. The intercorrelations of these measures were low positive with a median of .23; most were insignificant. The author concludes that "a global concept of adjustment, based on present day tests, is limited in usefulness." Consider now a study such as that of Hanlon *et al.* (66). These authors measured the congruence between self and ideal-self *Q*-sorts from a sample of high school students and found a linear correlation of .70, significant at the .001 level between this congruence and "adjustment" as measured by the Columbia Personality Test. Probably the inventory measures used by Tindall (the Heston Adjustment and the California Personality) would have given a similarly fairly high correlation with "congruence of self and ideal-self" in the present study. But, if we can generalize at all from one study to another (and note the similar ages of the samples), it is quite certain that Tindall's ratings and other "adjustment" measures would not have so correlated here. Probably at least a loose trait unity could be defined in terms of "inventory adjustment" or, better, "evaluative self-description," which would include the self-ideal-self correlation as well as the other common "adjustment" and "neuroticism" questionnaires (including the MMPI K and Pt scales and the MAS discussed earlier). And, clearly, "evaluative self-description" is not identical with what the clinician has in mind when he considers the adjustment status of his patient. We would consider the many investigations, of which that of Hanlon *et al.* is typical, to be studies not of relationships between psychological variables, as intended, but rather con-

tributions, albeit somewhat indirect, to the decision as to what the psychological variables really are.

Rigidity.—The concept of rigidity has enjoyed considerable popularity during recent years, and a number of studies have appeared demonstrating relationships between this psychological dimension and a variety of other variables. That this was a clear-cut case of putting the cart before the horse is illustrated by several studies during the period of this review which demonstrate that the various existing tests of "rigidity" do in fact measure quite different things and that "rigidity" as a trait or unidimensional variable is a figment.

Forster *et al.* (51) studied the relationships between performance on six types of problems supposedly affected by rigidity of set. Finding intercorrelations all of zero-order, these authors state ". . . our results point strongly to the conclusion that a person who displays behavior defined as flexible in one situation does not necessarily do so in another."

Wolpert (144) finds that intercorrelations between his five tests of rigidity range between $-.15$ and $.17$, in spite of odd-even reliabilities from $.69$ to $.95$ (rho's). He suggests, however, that the "rigidity syndrome" may coalesce under conditions of stress. That this is not apparently the case is shown by two other studies of similar tests under stress and nonstress situations. Applesweig (1) administered six rigidity tests to students in a submarine school, one group tested the day before an underwater escape test, one the day after this experience, and the third group a week later. Tau's calculated between all possible pairs of tests gave a chance distribution for all three groups of S's. Similarly, French (53) tested 50 airmen under "ego-involved" and 50 under relaxed conditions on seven tests of rigidity, finding "no evidence of a general interrelation between the various measures" and no evidence that the ego-involved condition increased either the rigidity scores or their intercorrelations. However, Pally (99), finding that S's do more poorly on the Luchins water jar problems under conditions of failure threat than under conditions of success, concludes that "rigidity" is an increasing function of anxiety. Unhappily, the preceding evidence forces us to deflate this interesting generalization to read that the relationship holds, at best, for "performance on the Luchins Jars."

Schaie (109) gave six rigidity tests, including two questionnaires, the Luchins water jar test, and three speeded perseveration versus flexibility tests, to a sample of 216 S's. The intercorrelations as before were generally positive but low, the highest correlation between any two "rigidity" measures being $.42$, between the two questionnaires. Nothing daunted, however, Schaie proceeded to factor analyze the matrix, finding a factor of "personality-perceptual rigidity" (the questionnaires and the jar test, loaded $.53$, $.57$, and $.31$) and a factor of "motor-cognitive rigidity" (the three perseveration tests). To test the invariance of this solution, the same battery was given to 200 college students and similar factors found, the only difference being a drop in the correlation of the factors from about $.45$ to about $.10$. Schaie

goes on to provide norms for his battery and equations for computing factor scores!

Some psychophysiological trait unities.—A very provocative paper by Wertheimer (140; see also 139, 141) provides an excellent example of at least the first phase of an hypothesis-guided pursuit of an underlying, organizing process. In this case, the process is a physiological variable, the efficiency of cortical metabolism. Wertheimer supposes that cortical satiation, hypothesized by Köhler to explain the figural after-effects phenomenon, may vary with the efficiency of cortical metabolism and that IDs in figural after-effects should correlate with differences in indices of metabolic efficiency both across people and, within individuals, over time. Certain predictions are generated and preliminary research is reported which tends to confirm them.

Thinking along very similar lines, Eysenck (42) has proposed a "theory of personality" based on the Pavlovian concept of cortical inhibition. A dimension of IDs based on a sort of hybrid of the Pavlovian and Hullian inhibition constructs is proposed as the basis of the introversion-extroversion trait; extroverts, e.g., hysterics, are assumed to develop inhibition more rapidly and enduringly. Eysenck, in contradistinction to Wertheimer and probably with equal legitimacy, interprets Köhler's "cortical satiation" in terms of cortical inhibition and concludes that hysterics should show relatively stronger figural after-effects. He reports a confirmation of this prediction in the kinesthetic modality. Other predictions are said to be undergoing test.

In view of the suggestion that CFF measures efficiency of blood sugar and oxygen supply to the visual cortex and, hence, may be related to general cortical metabolic efficiency, the finding by Chyatte (28) of a correlation of .85 between CFF and electroencephalogram alpha index may be of considerable significance to the Wertheimer hypothesis.

In general, this research would appear to be gradually fencing in a variable of brain physiology which also has at least some behavior relevance. Considering the apparent intraindividual variation together with the suggestion of constitutional differences, one might expect to see at least three indices or parameters emerge as the variable becomes more clearly defined, representing, e.g., the individual's momentary state, his range or variability, and his basal or mean value.

In the general area of brain pathology, which may or may not prove to be relevant here, Mednick (95) reports the extremely interesting finding that brain-damaged S's show distinctly subnormal stimulus generalization and the suggestion of similar but lesser deficit in schizophrenics. A peculiarity of the literature concerned with psychological deficit in brain pathology, illustrated in the recent review by Yates (145), is the curious absence of simultaneous correlational study of a variety of functions. Typically, an investigator reports significant differences between brain-damaged and normal individuals on some one performance measure. As a result, the clinician has no

rational basis on which to proceed when one test indicates damage and another not, and no clue is provided the scientist as to the number or nature of elementary psychological processes which may be affected by organic brain pathology.

Schlosberg (110) has discussed what is referred to variously as activation, arousal, alertness or awareness, level of energy mobilization, and, perhaps, vigilance (34). In consonance with Duffy, Lindsley, and others, he suggests that emotional reaction be conceived of in terms of an activation dimension together with separate dimensions of direction or quality. With respect to the latter, he considers his own research on judgments of facial expression to suggest tentatively a pleasant-unpleasant dimension and one of attention-repulsion. With respect to the activation dimension, he reviews various evidence supporting the use of electrical skin conductance as an indicant of the activation variable. That blood oxygen concentration may have similar psychometric implications is suggested in a study by Doust & Schneider (35) who report, e.g., a correlation of .43 between preference values for colors and the oximetric response to the colors. Deese (34) discusses the importance of a varied sensory input in the maintenance of activation or vigilance under monotonous conditions. Various performance measures seem to bear a curvilinear relationship with level of activation. Thus, Schlosberg indicates that reaction time speeds up as skin conductance increases through the part of the diurnal cycle from sleep to wakefulness but slows down again as conductance (i.e., activation) increases beyond optimum limits as with, say, stress. Possibly related here is the finding by Lybrand *et al.* (88) that performance on two tests of "perceptual organization" (viz., Kohs Blocks and Hidden Figures) improved after moderate physical exertion but worsened under conditions of extreme fatigue resulting from sleep deprivation. Thus, here again is the beginning of the delineation of a psychophysiological variable which holds promise.

King (83), in a model of a careful experimental study, studied the relationship between psychomotor function and what might be called the "severity" dimension of mental illness which has normality and deteriorated schizophrenia as its endpoints. The psychomotor domain is sampled with respect to three of the most clearly defined factors of fine motor skill, viz., reaction time, tapping, and finger dexterity (cf. the Fleischman studies above). His tests were carefully standardized on a sample of normal adults, using repeated testings sufficient to insure stable performance measures. His patient sample included 90 schizophrenics graded for degree of severity of abnormality according to three separate criteria, and 50 outpatients forming a neurotic and a pseudoneurotic or latent schizophrenic group. It is found that psychomotor performance becomes increasingly defective with increasing severity of illness. The pattern of low positive intercorrelation between the psychomotor factors is the same for both the normal and schizophrenic samples, but the marked and significant difference between these samples on

all tests indicates that these correlations would be much higher over the composite sample, as a result of the second-order factor related to severity of illness which has too little variance in any subgroup to be identified.

While this continuum from normality to schizophrenia accords with much common clinical thinking, one wonders what kind of dimension it represents. Is it a psychological variable of theoretical utility, a trait unity manifesting some underlying organizing process? It is such a question with which Eysenck is concerned in a pair of studies employing the new technique of canonical variate analysis (41, 43).

The specific problems with which Eysenck is concerned are (a) is there a continuum relating neurosis to normality and psychosis to normality or are either or both of these abnormalities categorically set off from the normal state, diseases *sui generis* like paresis or multiple sclerosis? (b) if the former is concluded, is there just one continuum relating normals to neurotics to psychotics in that order or are "neuroticism" and "psychoticism" separate dimensions? Previous factor analytic research (40) has led Eysenck to conclude in favor of the continuum as opposed to the categorical view. The paradigm employed involved the finding of a similar factor in separate factorizations for, say, a normal and a neurotic sample which, moreover, loaded the tests on which it was based roughly in proportion to their ability to separate the two groups.

Attacking the second problem, Eysenck (41) obtains normal, neurotic, and psychotic samples, administers four tests known to differentiate such groups, and then determines how many separate canonical variates (weighted sums of scores which maximize the correlation-ratio-squared across the three groups) meet the criterion of significance. Two canonical variates are obtained, from which Eysenck concludes that psychoticism and neuroticism are separate dimensions. A similar study (43) involving six tests and larger samples resulted in similar findings.

With respect to problem (a) above, we would point out that the research findings are quite compatible with the alternative which Eysenck sees them as disproving, viz., that neurosis or psychosis or both are "diseases *sui generis*," categorically distinct from the normal state (we do not, of course, argue that this alternative is true, but merely that this research has not proved it false). Using Eysenck's methodology, one could easily demonstrate a factor-analytically defined dimension, relating, say, tuberculosis and normality. Take, for example, a number of psychometrics having a weak common factor of "happiness" or "emotional well-being" running through them. One would then "discover" this factor independently in both samples, and moreover the tests would, *ceteris paribus*, separate the groups in proportion to their loadings on this factor. Which is simply to say that some normals are happier than others, some tuberculars happier than others, but tuberculars as a group, by and large, are less happy than people who are well. Nor is there anything special about "happiness"; "general physical endurance," "size of bank account,"—any number of others would do as well.

Eysenck's insistence upon using tests like dark adaptation, rail walking, and "leg persistence" on which his factors appear very weakly loaded makes it impossible to identify even vaguely the nature of the factors from the nature of the tests. To label them "neuroticism" and "psychoticism" merely because they tend to separate the respective groups seems at least premature since there are clearly many *ex post facto* differences between persons who are neurotic or psychotic and persons who are not.

With respect to problem (b), the independence of the neuroticism and psychoticism dimensions, much the same sort of argument holds. It is easy to think of variables which distinguish psychotics but not neurotics from normals (which is sufficient to duplicate Eysenck's results—his psychotics are high on both psychoticism and neuroticism). "Degree of delusional thinking" for example could be defined so as to have considerable variance within normal and psychotic populations separately and to differentiate psychotics from normals (and rather better than "dark adaptation," by the way) but not appreciably separate neurotics and normals. But does the identification of such a factor "prove" that psychosis is not a disease *sui generis* or that the essential pathology of the schizophrenic is not shared by the psychasthenic, and is the essential pathology but to a lesser degree in the latter's illness too? Clearly such a demonstration would require rather more than Eysenck has provided in these studies; it would require, indeed, a causal explanation of psychosis and of neurosis, neither more than this nor less.

General organic measures.—In spite of the generally unrewarding history of intercorrelation studies in this area, psychologists continue to work it over. Tyler (134) added another study to the many on "organismic" age with an analysis of the relationship between sexual maturity of boys (as determined by accelerated growth of the testes) and reading progress. A significant but low positive correlation was found. Tyler concludes, "Neither the theoretical implications nor the practical values of a coefficient . . . of the order here reported would be of much importance." Blommers, Knies & Stroud (14) made a frontal attack on the organismic age concept itself which is reminiscent of Paterson's earlier demolition work (101). They cited both old and new data showing low or negligible relationships between indices of "organismic age" on one hand and mental age and achievement on the other (with CA controlled). Then they performed variance analyses on the various indices of "organismic age" for groups of fourth, fifth, and sixth graders. The first component was the variance from child to child in mean organismic age (pooled height age, weight age, and dental age). The second component was the "within" variance; from one organismic age indicator to another within each child. The first component accounted for only 9 to 15 per cent of the total variance; the second accounted for all the rest. In short, organismic age is not by any means a trait or unitary construct and does not emerge with even statistical significance as a variable.

Sheldon, Dupertuis & McDermott (112) brought out an atlas of male somatotypes which includes 1175 examples representing the 88 somatotypes

identified in a total sample of 46,000 men. Personality descriptions and various related data are also presented. The book sheds little new light on Sheldon's many claims but should facilitate research in the body type area.

Interests.—The area of interests appears to be an undeveloped wilderness to the trait-oriented psychologist. Guilford *et al.* (61), cited above, is the only clear trait study in the two year period. While great amounts of practical data have been compiled for various interest test scores, little is known or even actively hypothesized about "interests" as constructs. As Strong (122) has pointed out, it is high time that the psychologist began attending to the construct as well as the tool.

Recent research has been largely aimed at discovering whether interest test scores (mainly SVIB scores) are stable for various populations for various intervals. The results indicate that they are. Powers (103) presents data for 109 "general population" men who were tested with the SVIB in 1931 and again in 1941. Individual test-retest profiles had a median correlation of .80. Scale test-retest correlations averaged .69. Stordahl (120, 121) used the SVIB with high school seniors and followed up a sample of 181 in college two years later. He found relatively high coefficients of concordance across test-retest profiles. Degree of similarity would not have been predicted by the Interest Maturity score of the first test, however. Letter grades C and A were most stable. Trinkaus (133) found similar results in the SVIB data for 212 Yale alumni, tested 15 years before as freshmen. He found good letter-grade stability (contingency coefficient of .58) with C and A grades most stable.

Strong's recent book (122) deals largely with a sample of 884 Stanford students in various academic groups who were tested in college, retested 6 to 22 years later, and for whom occupational data were available. The findings indicate considerable stability and predictive validity. For Ss in occupations for which there are SVIB scales the average student score was B+. The prediction was also viewed in another way: in this group of Ss there are 78 chances out of 100 that a man with an A rating in a specific occupation is in that occupation and 83 chances in 100 that a man with a C rating is not. Test-retest correlations for the scales vary around .70. Correlations of the test profile with the retest profiles (34 scales) yield median correlations of .72 (22 years) to .88 (one year). Strong also discusses the relationship of interest tests to occupations and the relation of interest and ability.

McArthur (92) and McArthur & Stevens (93) present some validity data for the SVIB and disclose an interesting "related" variable in a 14-year follow-up of 61 cases tested as Harvard sophomores. They found that both the Strong and expressed interests successfully predicted occupations for about half of the cases. The two techniques tended to "hit" for different populations, however. Boys from public schools were best predicted by the SVIB while boys from private schools were best predicted by expressed interest (which was in many cases the family decision as to the student's future job). The SVIB appears most adequate for the middle class "success" culture. The

studies are a good example of careful investigation teasing out what were at first "error" variables.

Levine & Wallen (85) compared Kuder Preference Records with current occupations of a group that had been tested 7 to 9 years earlier at the high school level. Men currently in occupations related to the original scales made significantly higher scores on those scales in high school than men in unrelated occupations. Herzberg, Bouton & Steiner (71) studied high school students retested two years later in college and found Mechanical and Artistic interests most stable and Social Service and Musical preferences least stable of the Kuder scales. Herzberg & Bouton (70) followed a similar group over a four year period. The Persuasive scale showed a lower correlation in the four-year follow-up (falling from .73 to .51) but other findings were very similar to the two year study.

Darley & Hagenah (33) summarize much that is of general interest concerning the stability of the SVIB and its validity. In a chapter of most direct concern they review 25 years of discussion, debate, controversy, and what data there are concerning the origin and development of interests. It is now widely agreed that this is a motivational construct, that it cannot be treated apart from the rest of personality, that it is not completely accounted for by any current theory, and that "it is unlikely that we can produce for some time to come any complete theory regarding the origin and development of occupational interests" (p. 190).

Tyler (135) is carrying out a developmental study which promises fruitful ways of conceptualizing the growth and development of interests. Her study of the likes and dislikes of young children reveals that the first differentiation is between male and female interests and that this is rapidly elaborated into perceptions of the kind of person one "ought" to be. This study will be followed with close attention by those involved in the area.

The problem of the origin and development of interests is a tantalizing one. The area seems to be ripe for effective trait isolation studies. Certainly the stability and the effective practical predictive power of interest measures suggest that some major, stable processes are at work here. But the ambitious investigator would do well to read Darley & Hagenah's chapter and Strong's cautions before launching an attack.

Miscellaneous traits.—In a study by Barron (2) of the "disposition toward originality" eight free-response tests of originality were administered to a sample of 100 Air Force captains. The intercorrelations between these tests were generally positive but low, only about half reaching significance and none greater than .46. The composite score correlated .55 with staff ratings of originality. The 15 highest and 15 lowest scoring Ss on this composite were then compared on an extensive set of questionnaire and rating variables to test various hypotheses concerning the relationship of "originality" to "personal complexity," "independence," "self-assertiveness," etc.

In criticizing this study, we would point out first that no intelligence measure is reported, a considerable oversight since, considering the nature

of the "originality" tests used, verbal intelligence could easily account for the weak common variance apparent in the intercorrelation matrix. Secondly, we are disturbed by what we regard as the author's cavalier treatment of the question of the functional unity of his "originality" variable. "If originality is indeed a dimension . . . we should expect the intercorrelations of these measures to be positive and to be statistically significant; we should not, however, expect the coefficients to be very high, for it is reasonable that the dimension of originality would have its variance apportioned to several media of expression." Setting aside all other questions, we must ask whether the investigator, having derived his composite score by the methods indicated above, is in a position to make any reliable or useful statements whatever about the important psychological construct "originality." Perhaps, in the last analysis, an answer can only rest on prejudice or faith, but in our opinion the answer must be in the negative. (Obviously, this criticism is predicated on the assumption that "originality" was being considered in this study as a functional unity. If "originality" is not a trait unity but merely a behavior category defined in terms of some social utility criterion, then it is our thesis that this attribute possesses little interest for the scientist as a theoretical variable.)

It is apparent that the general form of the Barron investigation is a familiar one (indeed, we regard this particular study as a rather good one of its type; it is the type to which we object). Dozens of studies appear in the psychological literature every year in which the relationships between important-sounding psychological variables are "investigated" by the correlation of certain highly specific, face-valid, often impromptu "measures." In the studies to which we refer, it is apparent that the authors regard the variables in question as important constructs of wide scope and that they by no means intend to restrict the meaning of these constructs to the narrow limits of operational definition in terms of the tests which they employ. And yet the absence, either in the study proper or in the literature cited by the author, of the necessary dimensional analysis or other evidence to aid in identifying the variable or in evaluating the method of measurement employed renders the "construct validity" of these measures negligible and the validity of the conclusions impossible to assess. A few recent further examples of this type of investigation include McBain's (94) study of "imagery" and "suggestibility," Steiner's (118) study of "ethnocentrism" and "tolerance of trait inconsistency," Sullivan & Adelson's (123) study of "ethnocentrism" and "misanthropy," Block & Petersen's (13) study of "some personality correlates" of "confidence" and "caution," Farber's (44) study of "anality" and "political aggressiveness," and so forth.

Other studies.—Because of the orientation of this review and the severe limitations of space, we have had to omit discussion of much other material of importance. Some of the publications which the differential psychologist will not wish to miss are listed here: Tyler's revised and now trait-oriented

book on human differences (136); Beach's (6) and Beach & Jaynes' (7) excellent reviews of the instinct "construct" and the effects of early experience on behavior; Fiske & Rice's (47) timely and much-needed reminder that intraindividual differences constitute an important and neglected part of the field; Bevan's review (8) of the literature on sound-precipitated convulsions which points out that to infer fundamental principles requires a knowledge of independent variables defined as dimensions; Fuller's monograph (56) presenting a biologist's thought on the nature-nurture issue; Christie's chapter (27) on a re-examination of "authoritarianism"; and Kelly's presidential address (81) on the consistency of the adult personality.

The phenomena reported by Thorndike (129) indicate real resistances to the trend we see and the course we recommend. In developing a value structure scale for psychologists he found three factors which appeared to account for most of the variance. The three were bipolars: (a) Laboratory versus Clinic; (b) Psychometric versus Verbal approach to the individual; and (c) Methodological Analysis versus Professional service. The correlation matrix shows quite clearly the very sizable correlations which keep psychologists apart. The scores on "laboratory values" and the scores on "study of the individual" negatively predict one another almost perfectly.

That this is deeper than a "data vs. verbal theory" split is shown by the fact that "laboratory values" correlate $-.80$ with the "collection of substantive data" (psychometrics) scores. Differential psychology must embrace all three points of view and perhaps will have to develop scientists who can combine what psychologists now see as incompatible values.

REPRISE

The aim of this review has been to emphasize (a) that the current literature betrays a continuing neglect by psychologists of the problems of taxonomy and (b) that the taxonomic endeavor requires resourcefulness and versatility both in theory and methodology within the framework of the "construct validity" approach. The process of trait identification cannot, of course, proceed in isolation. As the variables become more clearly defined, the laws relating them may be more clearly stated, and this in turn contributes greater specificity in the identification of the variables. We have argued that the taxonomic leg of this *modus ambulandi* has been allowed to atrophy and that psychology has hopped along on the other about as far as it can go. We do not believe that the taxonomic problem is "solved" by the device of identifying the constructs or variables with the particular operations used to measure them. As Bridgman himself has pointed out, "Definition of a phenomenon by the operations which produced it, taken naked and without further qualification, has an entirely specious precision, because it is a description of a single isolated event" (17, p. 248). The concept of construct validity seems to be the methodological antithesis of such a narrow empiricism but must not be prostituted into a justification for arbitrarily

assuming that whatever operations an experimenter has happened to employ are valid measures of the important construct which he wishes to discuss.

While this review has been critical, this should not be taken to imply that general solutions to the methodological problems are available. No panaceas are offered or should be inferred. We recognize that at some level in the development of a science, simple observation (i.e., almost "blind" data collection) is essential. But it is our hope that we are beyond that level in most of psychology. We recognize that many psychologists have service commitments and cannot wait for "pure" variables to be developed. We sympathize wholeheartedly. But it is our thesis that someone ought to be attending to the taxonomic questions—and that person is the differential psychologist.

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LEARNING^{1,2}

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At least four major trends have become more apparent in the past year. Mathematical models in learning seem to have grown in maturity and acceptance. The major landmarks are the appearance of Bush & Mosteller's *Stochastic Models for Learning* (22), the continued development of the Estes statistical model (44, 45), and the appearance of a chapter on mathematical models in a secondary text, Hilgard's *Theories of Learning* (72). Secondly, the year has seen a real resurgence in both theory and experiments dealing with the neurophysiological basis of behavior in general and learning in particular. Thirdly, the year has been marked by the appearance of a fairly rich variety of assumptions concerning the nature of reinforcement and the sharpening of some older statements. Fourthly, there is a growing theoretical and experimental emphasis on perceptual learning.

MATHEMATICAL MODELS AND LEARNING

There is likely to be a certain amount of confusion concerning the use of mathematics in learning as distinguished from the use of mathematical models and the additional question of when a model becomes a theory. With respect to the first distinction, the contrast might be seen in a comparison of Bush & Mosteller's model (22) and the mathematical treatment of learning data in Hull *et al.* (79). In his excellent review of Bush & Mosteller, Rapoport (121) classifies the *Mathematico-deductive Theory of Rote Learning* as empirical-descriptive since upon analysis the number of theorems hardly exceeds the number of assumptions. In contrast the Bush & Mosteller model makes a very modest number of assumptions and permits a larger number of theorems. A model becomes a theory only when the parameters and constants are identified or interpreted in terms of psychologically meaningful concepts and observables.

Bush & Mosteller's stochastic model.—In its current form this model is simple in that it involves the use of only two operators and two constants. The stochastic designation indicates that it is a model involving probabilities, or actually sequences of probabilities, in contrast to a model which is determinate in character. A large portion of the book is devoted to the development of the mathematical properties of the model, an aspect well covered in

¹ This review constitutes a rough and limited sample of the material published between April, 1955 and April, 1956.

² The following abbreviations and symbols are used in this chapter: ACTH (adrenocorticotropin); CR (conditioned response); CS (conditioned stimulus); EEG (electroencephalogram); GSR (galvanic skin response); *I_r* (reactive inhibition); nAch (need achievement); *I_r* (conditioned inhibition); Ss (subjects); UR (unconditioned response); US (unconditioned stimulus).

Rapoport's review (121). Five of the last seven chapters are devoted to the application of the model to particular experiments, and thus the test of its usefulness as a portable theory. The classes of experimental data involved include free-recall verbal learning, avoidance training with dogs, an imitation experiment with children, a series of symmetrical choice problems with both animals and humans, and some runaway experiments involving rats.

There is little doubt that Bush & Mosteller are offering a true mathematical model as indicated above. The more difficult questions are the extent to which it is a theory and its worth as a theory. They point out that their model does not require them to take sides in the contiguity versus contiguity plus reinforcement issue and specifically eschew, both in direct statements and in practice, issues of psychological importance. For example, they say, "Throughout this book we have attempted to divorce our model from particular psychological theories and have constantly talked about 'describing' data rather than 'explaining' it."

To the extent to which a model is identified or interpreted to concepts and data, it becomes a theory whether avowed or not. Despite the impressive power and elegance of the model, one might have wished for better identification and interpretation. At least the reader who is primarily a psychologist may end up somewhat confused and puzzled. When the model is applied to data from Solomon & Wynne (138) its major accomplishment is to produce a measure of the relative effectiveness of a shock trial as opposed to an avoidance trial in this study of avoidance conditioning in dogs. They find that an avoidance trial is worth about three shock trials. Roughly interpreted, this indicates that an avoidance is more likely to follow trials on which an avoidance occurs than trials on which the animal is shocked. When the model is applied to similar data by Brush, Brush & Solomon (20) in which CS-US interval was varied from 2.5 to 80 sec., this comparative value fluctuates considerably over the shorter intervals and at 80 sec. is of such size that a shock trial is worth about two avoidance trials. There is no particular effort to suggest why this difference might be so large; thus one perceives a failure of a needed interpretation.

In their application of the model to some data on imitation behavior collected by Schwartz (134) the interpretation seems somewhat inappropriate to the situation. In this study children were taken in pairs and asked to guess whether E would say *a* or *b* on each of 50 trials. One child always guesses second and in the experimental group was rewarded 80 per cent of the time for saying the same thing as the first child. A plot of the per cent of imitations in a group of 20 subjects starts below chance, rises irregularly throughout the series to a level somewhat above chance and shows sharp drops in imitative behavior following each punishment of this response. The model indicates that events which inhibit imitation are about three times as effective as events which increase it. Bush & Mosteller then say, "We might argue that this kind of copying is not approved in our society and hence the observed effect." The reader might be tempted to wonder what would have

happened if an additional group had been run in which nonimitation had been rewarded 80 per cent of the time. In this case one might expect that events which increase imitation might have been three times as effective as events which inhibit it, and that it could be argued as well that this kind of copying is approved in our society, as it certainly is in many contexts, and that this accounts for the observed effects. Thus it seems that we can add an error of commission to one of omission.

Thus the weakest link in the system is at the point of identification and interpretation, and this is less a criticism than a plea. From the standpoint of psychology, the model is nothing until interpreted, and the power and influence of models will depend upon the adequacy and meaningfulness of their interpretations.

Estes' statistical learning theory.—Two more theoretical articles have appeared which develop additional aspects of the Estes statistical learning theory (44, 45). They deal with spontaneous regression and recovery and distributional phenomena. Basically Estes distinguishes between the total population of stimuli which might be relevant to a stimulus situation, S^* ; a sample of this set which is available on a single trial, all of which are conditioned to the response in a single trial, S ; and those unavailable on that trial S' . With time there is a slow and random change of stimuli between S and S' . With massed training the transfer between S and S' should be minimal, and the result should be maximal apparent learning and maximum decrement after a period of rest. With spaced practice, the learning should appear relatively slow, and the decrement after a period of rest should be minimal. Also, with spaced practice, the total number of conditioned S 's should be maximal since a greater proportion of the S' , or unavailable population, will be conditioned; therefore, we can expect the spaced curve eventually to catch and surpass the massed curve. The reverse of these effects can be expected in extinction. When massing is extreme it is expected that the response will become conditioned to the aftereffects of the previous response and possibly to stimuli peculiar to a state of fatigue. When spacing is extreme, both habit strength and response strength might well be influenced by interpolated learning. Estes chooses to deal only with experiments involving intervals of medium length in order to avoid these factors not yet incorporated in the theory. There is, however, an experiment by Lauer & Estes (91) on repeated conditionings and extinctions under 100 per cent and partial reinforcement in which some of the predicted effects are attributed to differences in the cue population following reward as opposed to nonreward. One would suppose that this study signals the eventual elaboration of the theory to incorporate changes in the stimulus sets attributable to a variety of factors of psychological importance other than random fluctuation. The random fluctuation hypothesis does, however, predict some of the findings in the Lauer & Estes study as well as some of those reported by Homme (74). The latter suggests that his data might be better fit by a modified assumption allowing for a common core of stimulus elements which

remain fixed while the remainder of the population fluctuate randomly.

One article by Estes & Burke (46) appeared during the year which reports an experiment specifically designed to provide an application of the model to simple discrimination learning in human subjects. In it there are two sets of stimuli (half of a circle of 12 constitutes a set) and two responses, that the reinforcing light will or will not come on. One set is reinforced 100 per cent of the time, the other 50 per cent. The two critical assumptions are that the response which terminates the trial is conditioned to the cues sampled on the trial and that the probability of a response on a given trial is directly proportional to the proportion of the elements conditioned to the response. Off hand it would appear that the six lights of each set constituted the total sample in the set, in which case conditioning in the 100 per cent case would be complete on a single trial, and with the other set the response would be completely conditioned or extinguished each time it was reinforced or not reinforced. The expected appearance of the curves is quite clear. The set sampled, however, is a much more molecular population, with the result that the curve can be expected to approach the asymptote gradually. There is no way to determine independently the proportionality between the set sampled and the population from which the sample is drawn. Without an additional assumption one would expect one curve to be asymptotic to 100 per cent and the other to 50 per cent. However, it is assumed that some of the stimuli sampled on each trial are common to both sets, and such stimuli are reinforced 75 per cent and not reinforced 25 per cent of the time. This set of common stimuli will have the effect of making one curve asymptotic to a value somewhat below 100 per cent and the other to a value somewhat above 50 per cent. There is no way to estimate the proportion of common stimulus elements to the total population from which each sample is drawn. Therefore, neither the rate of approach to the asymptote nor the asymptote itself can be determined in advance. Both values must be determined after the data are collected.

Other difficulties appear to emerge from examination of very similar studies which have appeared during the year. In studies by Morin (109) and by Wyckoff & Sidowski (160) it is indicated that the curve for the group does not seem to represent the behavior of any large proportion of the subjects. With a given proportion of reinforcement, say 75 per cent, many subjects proceed rather directly to 100 per cent selection (optimum) behavior. Others seem to adopt a frequency of choice somewhat below the probability matching point (75 per cent), and the mean of the group falls somewhere above it. Furthermore, in a study by Goodnow & Pettigrew (62) shifting subjects from 100:0 to 50:50 and to either 0:100 or 100:0 seemed to produce learnings sets which resulted in very quick shifts to optimum behavior in most subjects.

Advantages and disadvantages of mathematical models.—Many of the virtues of mathematical models have been specified by their originators and listed by Hilgard (72). They are several and they are real. In brief, the model sum-

marizes and permits reproduction in considerable detail of most of the original data.

Some of the disadvantages are equally real. (a) Mathematical formulations have high prestige value, and there is esthetic value in apparent precision and the elegance involved in a small number of assumptions and a large number of theorems. This in itself is a mathematical and not a psychological virtue. If the character of the model is largely determined by mathematical rather than psychological considerations, it is possible for it to be mathematically parsimonious without being psychologically parsimonious or even meaningful. (b) At their present stage of development, both models seem to predict what has already been obtained better than what might be attained in the next experiment. This seems to result from the fact that the identification is to variables which cannot be independently measured and can only be estimated after the experiment is over. (c) At least in the Bush & Mosteller instance the studied and deliberate emphasis on the model seems to lead to a failure to treat the psychological aspects of the experiments with anything like the level of sophistication exhibited on the mathematical side. (d) Given the present stage of development in the field of learning, one wonders whether both models might not become unmanageable agglomerates if attempts were made to expand and develop them to encompass even a limited set of the variables which are of interest to the experimentalists working in the area the models were designed to fit.

Both models represent significant contributions to the field of learning, a significance which will tend to grow in the immediate future. In this review no limitation has been mentioned of which the originators are not aware. Psychologists, even old ones, had better accelerate their study of mathematics.

NEUROPHYSIOLOGICAL VARIABLES AND LEARNING

The growing theoretical interest in neurophysiological variables was landmarked during the year by Hebb's explicit incorporation into his system of the well documented distinction between the specific and nonspecific effects of sensory stimulation (69). The distinction is between impulses transmitted to specific projection areas in contrast to a somewhat slower, low level spread of activity delivered rather indiscriminately to widespread cortical areas. The latter involves activity in Magoun's ascending reticular system, activity identified with arousal or vigilance. Hebb proposes that arousal in this sense is synonymous with a general drive state and further suggests that there is an optimal arousal level. Increases from lower levels of arousal toward the optimum would be rewarding, as would be decreases from levels higher than the optimum.

This identification between psychological and neurophysiological variables, both of which can be measured, opens exciting possibilities which will undoubtedly be exploited in the near future. For example, it is well established that proprioceptive return involves an exceptionally high proportion

of nonspecific input as compared to visual and auditory stimulation. Shaw (133) has reported the effects of induced muscle tension on perception span for digits. He finds that a certain amount of muscle tension increases the perception span for digits and that the optimum tension increases with the difficulty of the problem. It is tempting to hypothesize that the effect is attributable to increased arousal, and so to bring into reconsideration the vast body of data on inter-modality interaction, the effects of "distraction" on learning, and the early studies on muscle tension and learning.

Brain stimulation.—It is also tempting to guess that to some extent at least the studies of the effects of brain stimulation can be incorporated in a coherent picture of the effects of differential arousal. Gengerelli & Cullen (53) have reported a study in which rats received cerebral stimulation during the process of formation of a black-white discrimination habit. The study shows facilitation of learning with two different rates of stimulation, 75 and 300 per sec., with more effect with the higher frequency.

Endocrinology and learning.—There are a number of studies which are somewhat removed from the specific and nonspecific distinction in neural function, but which concern themselves to a degree with possible differential effects upon habit formation as opposed to effects upon motivation. Stone & Obias (142) show that hypophysectomized animals are significantly inferior in maze performance to controls but feel that they cannot distinguish between loss of ability as opposed to a disturbance in motivation. Applezweig & Baudry (4) have shown that hypophysectomy in the rat reduces the efficiency of acquisition of an avoidance response, a deficit partially restored by ACTH injection, but again the locus of the effect is not clear. Kosman & Gerard (88) seem to have shown that the detrimental effect of epinephrine (adrenaline) injection is peripheral rather than central. They taught rats to press a lever at a light signal to avoid shock. Epinephrine-injected animals showed a profound loss in performance compared to controls. A group injected with epinephrine and treated with dibenzylamine showed no loss, and dibenzylamine has no known central effect and is assumed to block the peripheral effects of epinephrine.

Electroshock convulsions, perseverative consolidation, and fear.—A series of studies involving the effects of electroshock convulsions on learning seem to have produced a clear and concise resolution of the problem of differential effects upon habit as opposed to motivation. There are early classic studies by Duncan (38, 39) which demonstrate attenuation of an acquired habit through application of electroshock convulsions. His data suggested clearly that it was only the last habit which was affected. The perseverative consolidation hypothesis of memory has recently been returned to the foreground by Gerard (54) who thinks of it as being reverberatory. Walker has recently reported reaction decrements lasting 90 min. or so (153) and has suggested that they depend upon some form of continued neural activity in the reaction system. He currently favors a mechanism in which the reaction system is maintained or driven by nonspecific input. However it is maintained, both

hypotheses require a consolidation period after learning for maximal retention.

Thompson & Dean (146) have reported a beautiful study in which animals were given massed training on a simple discrimination problem. Four experimental groups were subjected to electroshock convulsions 10 sec., 2 min., 1 hr., or 4 hr. after the completion of training. The first three groups showed clear deficits inversely related in amount to the interval. The 4 hr. group did not differ from the controls. These results seem to demonstrate interference with some perseverative consolidation process. The intervals coincide precisely with the duration of the reaction decrement which Walker has reported and which he has postulated is positively related to habit strength. On the other hand, Geller, Sidman & Brady (52) offered a different interpretation of Duncan's results, and they appear to be equally plausible. It is their thesis that electroshock convulsion attenuates a conditioned emotional reaction. They attacked the problem of order of habit acquisition by setting up a conditioned emotional response to a clicker stimulus by pairing it with shock. The next period of training, which occupied seven days, involved pressing a lever for water reward. Beginning on the following day there followed three electroshock convulsions per day for seven days. They then demonstrated that the lever pressing habit was unaffected by the electroshock convulsions, but the conditioned emotional reaction to the clicker stimulus was virtually absent in the animals subjected to electroshock convulsions. This effect of the convulsions upon the conditioned emotional reaction apparently dissipated over a 30 day period. Heistad (71) has shown similar results. It seems clear then that electroshock convulsions applied within the first hour or two after the acquisition of a habit will interfere with the perseverative consolidation of the habit. It seems equally clear that if the performance of the habit is motivated by fear or anxiety, electroconvulsive shock will produce a temporary attenuation of the performance regardless of when it is applied with respect to the period of acquisition.

Electroencephalography.—This technique provides a tool which should be of increasing interest as psychological and neurophysiological theory develop by increasingly coordinate definitions. Ellingson (42) has written a review of this field with a bibliography of 206 items, many of which are directly relevant to learning problems. The EEG provides one index of the degree of arousal and thus a measure relevant to Hebb's theory of motivation and reward. Ellingson reports numerous studies of conditioned EEG patterns which can presumably be interpreted as conditioned arousal, conditioned motivation, or conditioned inhibition. There is no point in reviewing Ellingson's review here, nor has the attempt been made to cover all of the relevant literature which has appeared during the year since most of it is published outside of normal psychological journals. A sample, however, might suffice. Morrell & Jasper (110) have reported a study in which CS's in several modalities have been employed in monkeys. The US was a flickering red light and the UR was the blocking or photic driving of the EEG.

This blocking response is identified with arousal. Conditioning appears quickly and in all of the widely separated leads indicating a generalized arousal of the whole cortex. With further trials the area showing this arousal pattern "contracts" to the occipital zone. Simple conditioning reaches criterion in this study in an average of about 10 or 11 trials. It is easy to predict that since the groundwork is so well laid, we can expect to see more and more use of the EEG in studies of learning and motivation.

Brain damage.—There are many studies reported during the year which relate damage to the central nervous system in various degrees of specificity in location to learning and performance. Typically they relate damage in a given area to performance, and there is a persistent question as to the precise nature of the effect—on capacity to discriminate, on habit, on learning capacity, on response control, or on motivation in general or a particular kind of motivation.

There is not sufficient space for review here, but the patient plotting of the topography of the central nervous system by a method of ablation or damage and the observation of behavioral effects shall be with us for a long time.

SYSTEMATIC ISSUES

Most of the articles which have appeared relevant to systematic issues have dealt with some aspect of the reinforcement process, its conceptualization and interpretation.

Elicitation theory.—Denny & Adelman (35) have proposed that a system can be profitably built which analyzes no deeper into learning and performance than the detection of the response and the stimuli which elicited it. Consistency of elicitation is the key notion. It remains to be seen how much power can be generated by such a parsimonious system.

Reinforcement and context.—Perkins (118) has proposed that rather than to assume some such condition as drive reduction as being reinforcing, profit should adhere to the assumption that whatever is followed by a relative selection of a response is reinforcing. His subsequent analysis places an emphasis on patterning of reinforcing situations and context. Within this framework knowledge of the reinforcing effects of stimuli in one situation does not permit direct prediction of their reinforcing properties in another. This analytic address to reinforcement seems to offer a new approach to the extension of the concept to human learning where the simpler conceptions of reinforcement do not seem particularly useful.

Mowrer's hopes and fears.—Mowrer (111) has abandoned his old two-factor theory which categorized all learning into sign learning and problem solving. First, there is acquired fear. Fear is no longer considered to be a conditioned form of pain but is held to be a part of the unconditioned response to noxious stimulation. When a neutral stimulus is associated with the onset of pain and fear, the fear becomes conditioned but the pain does not. Second, there is conditioned hope. A neutral stimulus associated with a

decrement in drive comes to produce a conditioned decrement. There is some uncertainty whether conditioned hope is a reduction in primary fear while the noxious stimulus is still present, or whether it is a reduction in conditioned fear. There are now two types of secondary reinforcement: that occasioned by the removal of a stimulus with acquired fear properties in addition to directly conditioned hope.

Hebb's conceptual nervous system.—As indicated elsewhere, Hebb (69) has clarified his previously stated position or extended it by identifying drive with arousal or vigilance in the neurophysiological sense. With Perkins (118) Hebb implies that knowledge of the stimulus effects in one situation alone is not enough to specify its reinforcing properties. What is rewarding will depend upon the current state of arousal. There should be an optimal state such that a reduction in arousal toward it is rewarding (drive reduction) and an increase toward it (stimulus hunger?) should also be rewarding.

Tolman and principles of performance.—Tolman (148) has given his theoretical vehicle a general overhaul and has reissued it in stripped-down form. The "schematic sowbug" reappears in a simplified version, and Tolman draws some rather direct comparisons between his own formulation and that of Hull. However, none of the broad principles upon which Tolman has based his thinking in the past has been violated. To this reader it seems that one new thing has been added. It has been suggested that the old model could not start by itself and had to be cranked. In this version he borrows from Glanzer (58, 59) a need-cognition or need to examine new stimuli which involve approach and manipulation movements, a self starter. As this need dissipates, as it will with growing familiarity, the "need pushes" present and "valences" acquired with respect to objects present will take over and run the sowbug. The bug is now a self-starting hot-rod.

Hull's system.—Attack, defense, and adjudication of Hull's system (76,77, 78) proceed. Brown (18) has issued an able and lengthy defense of the drive reduction hypothesis. He reviews the studies which have been taken as being critical of the hypothesis and disposes of some as presenting no good evidence that they constitute an increase in drive. He disposes of others by saying that a drive increase can have the function of augmenting the on-going behavior. When disruption does take place, he attributes the effect to the elicitation of an interfering response by the drive-produced stimuli rather than to any negative motivational attributes inherent in them. Seward (131) has published a critique of intervening variables in which he does not find what he considers a sufficient degree of constancy in them as of the present. Elsewhere (132) he develops a cogent argument that given Hull's redefinition of habit strength and his use of goal anticipation as the major determinant of reaction potential in the latest revision of his system (78), the difference between reinforcement and expectancy theory has become minimal. Restle & Beecroft (127) analyze a series of eyelid conditioning experiments and demonstrate a high similarity in prediction between the Hull-like system of Spence & Farber (139) and that of Bush & Mosteller (21).

Cotton (28) has challenged whether there is the degree of correspondence between Hull's postulates and his empirical equations which is ordinarily assumed. He makes clear that a literal interpretation of certain of Hull's postulates leads to predictions which are not in agreement with the empirical equations. His article is also a pithy treatment of the problem of prediction from theory in general. On the other hand Birch (13) has performed a neat experiment on the ratio of forced reinforcements to nonreinforcements to the positive and negative cues. He derives two independent measures of effective excitatory potential differential, one based on latency and the other on per cent of correct choices, which are in remarkable agreement.

Finally, Logan (93) has amended the response side of Hull's theory by developing a micromolar approach to the response. He feels that the response side is as differentiated as the stimulus side, that different speeds of response, for example, can be treated as different responses, and that different excitatory potentials can be assigned to them.

MOTIVATION AND REWARD

Drive and performance.—French (51) has succeeded in demonstrating a relation between individual differences in nAch scores and performance on a digit-letter substitution task. On the other hand, Haner & Brown (68) report a clever little experiment in which performance is taken as an index of drive. They give children the task of setting 36 marbles in a board, and the marbles are offered one at a time. At various points in the task, the marbles already set are dropped through the holes and the children have to start over. When the marbles dropped, a buzzer sounded until the child pushed a plunger to turn it off. Strength of push was taken as a measure of the frustration and instigation to aggression produced, and it was found to vary as a function of the number of marbles in place at the time of the drop.

Conditioned drive versus conditioned avoidance.—Hunt & Brady have attempted to disentangle the acquired drive from specific avoidance responses (80). In one group they paired a CS with shock and were careful that the animal was not shocked during a lever pressing response. In another group they shocked the animal only when it pressed the lever during the CS. They found that both procedures led to suppression of lever pressing in subsequent tests and found that the group punished during the pressing showed fewer signs of fear, extinguished the fear more rapidly, and showed more indications of conditioned avoidance than the other group.

Shock onset versus offset studies.—Mowrer's position (111) reemphasizes the different expectations from pairing a neutral stimulus with shock onset as opposed to offset. Davitz (33) paired a CS with either onset or offset of shock and found more fear-associated activity in the onset group in the test situation. Gleitman (60) found that when shock onset was associated with one place and offset another, animals given a subsequent choice tended to choose the place of offset but showed no preference between either place and a neutral place. Goodson & Brownstein (63) tested rats for preference

for black or white end boxes from a central restraining chamber. They then shocked the animals in one end box and permitted escape to the other. They found a preference for a neutral as compared to the shock chamber and a preference for the escape as compared to the neutral.

Amount and delay of reward.—There are several studies on the amount and delay of reward which are sophisticated in the sense of predicting something beyond more learning for large rewards and short delays. Logan, Beier & Ellis (94) varied both amount and delay. They predicted and found that when the delay of reinforcement is varied between two equally likely values, asymptotic performance is comparable to constant reinforcement at the preferred (shorter) of the two values and significantly superior to constant reinforcement at the mean of the two values. On the other hand, when the magnitude of the reinforcement is varied between two equally likely values, asymptotic performance is comparable to constant reinforcement at the mean of the two values and significantly inferior to constant reinforcement of the preferred of the two values. This prediction is made from the assumption that in the case of the two delays the fractional anticipatory goal reactions are the same, differing only in intensity, while with two magnitudes of reward they are different and incompatible. Dufort & Kimble (37) investigated the effects of shifting amounts of reinforcement during learning on the slopes of performance curves. They confirmed their predictions from Hullian theory. Campbell (24) used white noise as a noxious stimulus and determined the degree of difference between two noises required to produce 75 per cent choice of the lower. In this manner he determined "reinforcement fractions" ranging from .74 to .96 which compare in constancy with the Weber fractions.

Secondary reward.—Hopkins (75) trained rats in a discrimination problem with different amounts of reward (.05 to 2.4 gm.) and saccharine. Subsequent testing of preference indicated that the positive goal box had acquired secondary reward value, but there were no differences between groups. D'Amato (30), on the other hand, obtained positive results. He trained on a straight runway with a distinctive goal box containing five pellets on half the trials and another with one pellet on the other half. Subsequent tests of preference showed a high level of selection of the goal box associated with the higher reward.

In a further experiment D'Amato (29) trained to a distinctive box in the straight runway under hunger and food reward and tested preference under thirst. In another group the motivational relations were reversed. His results suggest generalization of secondary reinforcement between the two drives.

Schroder (129) varied percentage and frequency of reward independently in young children to establish the secondary reward value of tokens. Partial reinforcement served to maintain the secondary reward value of the token over a greater number of extinction trials.

Reaction decrement and exploratory drive.—Few conceptions in psychology have stimulated more intensive assault than Hull's formulation of inhibition

(76, 77, 78). Reactive inhibition, I_R , had four major characteristics: (a) the amount of I_R was positively related to the amount of work, (b) I_R was a response variable as opposed to a stimulus or central variable, (c) I_R was assumed to decay quickly as an exponential function of time, and (d) I_R reduction was presumed to act like drive reduction and produce semipermanent inhibition, sI_R , which accounted for extinction. No one of these four characteristics has survived further scrutiny unscathed.

At least as they apply to alternation behavior in rats the work variable has been pretty well disposed of. Variations in work do not seem to produce variations in alternation [for example, see Montgomery (104)].

In addition to several previous similar findings, Estes & Schoeffler (47) find that there is no alternation with respect to response when the situation does not permit alternation with respect to stimulus and place. On the other hand, Walker *et al.* (154) have demonstrated that if the afferent returns from two responses are made highly distinctive, response alternation can be produced. Glazer's (58, 59) reformulation in terms of stimulus satiation fared no better in this respect since Walker *et al.* (155) found simple stimulus exposure insufficient, and Estes & Schoeffler (47) found that alternation did not generalize to other turns as would be expected with a stimulus satiation hypothesis.

When Walker (153) plotted the course and duration of the decrement from a single choice, he found it to last 90 min. or more and to give the appearance of being a sustained process followed by a rather abrupt decay when plotted against log time. At least the decay curve departs radically from a simple exponential decay function.

Finally, Walker (153) has proposed that the amount of the decrement is positively related to the amount of the increment or habit strength, a proposition for which there is as yet little evidence. However, Bilodeau (12) using a crank turning task and Ammons & Willig (3) using a rotary pursuit find no evidence of permanent work decrement. Bourne & Archer (17) also using a rotary pursuit device find that massing may result in a semipermanent loss, but they attribute it to an interference with original learning attributable to the temporary work decrement rather than to a learned inhibition.

If decremental phenomena are more widespread in their application, and if they are not specifically associated with work, what are they and how are they to be explained? In an excellent review of variability including coverage of the area at hand, Fiske & Rice (49) assert their faith that intra-individual response variability is not random; it is a lawful phenomenon. General agreement seems to reach no farther.

One basic problem seems to be whether alternation phenomena are to be explained as decrements in something or increments in something else, or a combination of both. Walker has argued (153) that when an organism performs reaction A, the system involved undergoes a decrement of limited duration during which B is a likely choice, assuming original equal attractive-

ness of the two alternatives. Until it is chosen, nothing is presumed to happen to the threshold for alternative B. However, if some aspect of the situation is changed between choices, a new factor, novelty, has been introduced which has a powerful influence on behavior. This has been clearly demonstrated by Kivy, Earl & Walker (87) and by Dember (34). The attractive powers of novel stimuli have been further demonstrated by Butler & Alexander (23) who measured the tremendous drive of a monkey to see out of a box; by Berlyne (11) who developed a rather precise measure of the attractive powers of a cube placed in a familiar environment; and by Hebb & Mahut (70) who demonstrated that rats would persist in choosing a long rather than a short path as long as the long path presented new problems to solve. Hill (73) showed that differential hours of restriction of activity produced differential amounts of activity-wheel turning activity. Kish (86) and Marx, Henderson & Roberts (101) have gone farther. They both showed that animals, such as rats and mice, would press a lever to turn on a light. These researchers interpret their results as attributable to stimulus change or novelty as reinforcement. Cole & Caldwell (27) have performed a similar study with goldfish.

Montgomery & Segall (108) have demonstrated T-maze learning in the rat with the reward being an opportunity to explore a complex Dashiell-type maze. Montgomery & Monkman (107) and Montgomery (106) have concluded that exploratory behavior is motivated by an exploratory drive induced by novel stimuli and that fear will inhibit exploratory drive because the two are in conflict. Both studies present data which can be so interpreted. However, Montgomery's similar finding with respect to hunger and thirst (105), namely that exploration was inhibited by the presence of either, is contradicted by Adlerstein & Fehrer (2). They compared hungry and sated rats in a complex maze and found that hungry rats explore from 50 per cent to 75 per cent more than sated animals. This agrees with the classic finding of Dashiell (31). A possible resolution exists in the study by Campbell & Sheffield (25) who found that hunger lowered the threshold for effectiveness of novel stimuli, a principle used by Adlerstein & Fehrer. Both they and Dashiell used complex fields while Montgomery used a simple one. It may be that an increase in activity might be seen in hungry and thirsty rats in Montgomery's situation only as long as the stimuli are novel and a decrease when they become familiar. A real resolution of this apparent contradiction at the data level will require further thought as well as further experimentation.

Estes & Schoeffer (47), in addition to their presentation of critical data, offer an alternative explanation of the phenomena. They suppose that animals are normally differentially reinforced for approaching novel stimuli as opposed to familiar ones in the laboratory environment. They reject all other explanations on the basis of parsimony.

A variety of variables have been shown to influence the amount of alternation. Walker has shown (153) that reward serves to increase alterna-

tion rather than to decrease it. Goodnow (61) in dealing with response sequences in two-choice probability situations contributed a new term "recency effect" and showed that the phenomenon existed in the sequence of responses if it is a "gambling" situation but not if it is seen as a "problem solving" situation. When her data are converted to alternations, there is some evidence that the recency effect after a reward is greater than after nonreward.

Malmo & Wallerstein (97) interpret rigidity as the result of weak reactive inhibition and demonstrate that psychoneurotic patients show less decrement in a repeated voluntary response than do normals. Thompson & Kahn (147) show that bright rats reduce their rate of exploration faster than do dull rats in a simple environment. One wonders what the picture would be like if a complex environment were used. Payne & Hauty (117), using human subjects and a complex compensatory pursuit task, found that variation in information feedback or knowledge of results and variations in motivation had either negligible effects on work decrement or at best delayed the effects briefly. Drugs on the other hand produced significant delays in the onset of work decrement or significantly hastened the onset depending on the drug.

PERCEPTUAL LEARNING

One of the hoariest problems in learning seems to be undergoing some degree of clarification, both theoretically and experimentally. One way to state the question is to ask whether learning consists primarily of perceptual reorganization, association, or differentiation; primarily of associations between stimuli and responses; or chiefly of differential acquisition and extinction of secondary drive and reward value accruing to elements which are taken as given.

Gibson & Gibson (55) have made a strong case for some sort of differentiation occurring in perception as a function of repeated experience which consists of the progressive elaboration of qualities, features, and dimensions of variation. Postman (120) has challenged this notion and has reasserted the old associationistic notion that perceptual learning is a problem of behavioral change and experimentally reduces to the study of stimulus-response associations. Gibson & Gibson (56) have replied and reaffirmed their specificity theory. The cogent discussions on both sides of the issue have helped to clarify for this reviewer what he considers the major loci of difficulty. First, there is a fundamental confusion stemming from the fact that there seems no way to escape the condition that a response must ultimately be used as an index of a change in perception. If so, how can Postman's position be refuted? The answer may lie in clever experimental designs in which the index response must shift its association from trial to trial with respect to the particular aspect of perception presumed to be changing. The present reviewer is willing to predict that such designs will appear and make Postman's position difficult to defend.

The second difficulty resident in the position of Gibson & Gibson (55)

is the specification of what is learned, which they have attempted, and which must occur before techniques of measurement can be applied that will make their position testable. One likely source of aid might be information theory which seems peculiarly equipped to describe the difference between a differentiated and a less well differentiated percept.

Murphy (112) states a position akin to that of Gibson & Gibson and chooses to emphasize the influence of affect upon perceptual structure. He suggests that perceptual structure changes in such a fashion to give emphasis to that which is pleasant in contrast to that which is unpleasant or affectively neutral and that many such changes are cumulative and constitute a form of perceptual learning.

Mowrer (111), in his new theory discussed elsewhere, rejects both perceptual learning and stimulus-response learning and suggests that all learning is in the nature of sign learning, significance learning, meaning learning—conditioned increments in drive (fear) and conditioned decrement (hope). At least the lines are drawn and clarifying facts seem somehow more imminent.

An example of the use of amount of information as an index may be seen in a study by Attneave (6). He did an experiment on memory for pattern in which he compared symmetrical and asymmetrical patterns containing either the same or different amounts of information. Very roughly he found that the amount of information was more important than symmetry per se. Detambel & Stolurow (36) offer a concept learning experiment in which the pattern of presentation of the stimuli could be indexed in terms of amount of information. They found that bright Ss were not particularly hampered by synchrony or minimum information, while less bright Ss were hampered.

There are two studies demonstrating the effects of training on perception which are difficult to assign to the stimulus-response learning category. Weiner (156) provided Ss with several varieties of training in the perception of the up-right and demonstrated positive transfer. Gibson, Bergman & Purdy (57) gave Ss training in a scale of distance and then tested their ability to make absolute or relative judgments of distance in another situation. They demonstrated improvement in the absolute judgments but not in the relative judgments.

Sidman (136) taught rats a lever-pressing escape response from shock scheduled to occur 20 sec. after any pressing. The introduction of a CS 5 sec. before the shock served to reduce the number of responses by about one half. Sidman interprets some of his results as evidence of improvement in temporal discrimination.

Generalization.—Guttman & Kalish (66) plot generalization gradients in pigeons against wavelength and produce quite regular curves. The absence of distortion where there are shifts in hue or across areas which vary in discriminability leads them to suggest an independence between discriminability and the generalization decrement.

Gleitman (60) demonstrated place learning without prior performance by

shocking rats while they were riding in transparent cable cars and then demonstrated significant avoidance of the starting point as opposed to the terminal point of the ride. There was no significant tendency to choose a neutral point as opposed to either the start or terminus. This study seems to demonstrate perceptual learning of a kind, and Brown & Humphrey (19) not only demonstrate the efficacy of place learning but demonstrate generalization of a spatial sort.

Duncan (40) reports the development of response generalization gradients developed through differential rates of extinction of errors in which a lever is to be moved into one of six horizontal radial slots.

Discrimination learning.—There are a number of new studies on transposition phenomena. Thompson (145) trained rats on two discrimination problems simultaneously, on circle size and on a pair differing in reflectance. Half could compare the circles simultaneously, for example, half had the positive circle with the negative grey. Thompson found more transposition under comparison conditions than under noncomparison conditions, and more with the difficult than with the easy problem. Stevenson & Bitterman (140) trained children from 4 to 6 to discriminate the intermediate member of a set of three stimuli differing in size and found transposition to a near set but not to a far set. They argue that neither a relational point of view nor one based on learning to absolute properties can encompass their results and argue for two relational processes, one which is abstract (verbal) and one which is more closely bound up with the absolute properties of specific situations. Stevenson & Weiss (141) report that transposition in college students is positively correlated with a failure to notice that the test pair is different from the training pair.

There are a whole host of studies seeming to point to the complexity of the discrimination learning process. There are four complex studies involving rats all of which seem to argue that it is really a variety of things which are being learned simultaneously in discrimination problems. North & Jeeves (116) and Bitterman, Tyler & Elam (15) come to such conclusions in comparing simultaneous and successive discrimination training and transfer. Birch & Vandenburg (14) with a complex design conclude that while there has been some learning to absolute properties of the stimuli there has also been learning to patterning of a type which Spence would not consider possible. Goodwin & Lawrence (64) seem to have demonstrated the perseveration of an acquired discrimination habit during random reinforcement in a way which would not be expected from a continuity position. The authors think the animals learn first and most rapidly which stimuli are relevant, and then learn more slowly which stimulus is positive. If the animals are shifted to new stimuli, they learn that the new stimuli are relevant before complete extinction has occurred with respect to which of the old stimuli was positive. This unextinguished habit strength can be demonstrated as savings in relearning the original discrimination habit.

Eriksen & Wechsler (43) compared a normal group with a shocked one on

a discrimination task. They found that the stress did not influence the capacity to discriminate, but the shock group showed more bias in the use of response categories. Kurtz (90) demonstrated that transfer of discrimination from one task to a second task was positive when the stimuli were distinguished by the same property on both tasks, and that transfer was negative when the stimuli were distinguished by different properties on the two tasks.

VERBAL LEARNING

A number of studies have appeared in the past year with such diverse interests that they permit little in the way of grouping or integration.

Distribution of practice.—Underwood has published the thirteenth and fourteenth in his series of studies of distributed practice. Underwood & Richardson (150) tested the hypothesis that massed practice would facilitate retention when interlist interference is low and distributed practice when interlist interference is high. Using only materials with high interlist similarity, they had half of 168 subjects learn under massed practice and half under distributed. The subjects learned five additional lists under massed and a seventh with half massed and half distributed practice. Distributed practice facilitated acquisition on both lists, but when tested for retention 24 hours later, it was found that massed practice had produced better retention of list 1 and the reverse on list 7. Underwood & Archer (149) varied level of interlist similarity, rate of presentation, and distribution of practice in verbal discrimination learning. They found that the lists of low similarity were learned and relearned faster, that slower presentation speeds facilitated learning and relearning, and that rate of learning and relearning were unaffected by intertrial interval. Recall of the high similarity lists, however, was better. These studies are interpreted as requiring more than a simple inhibition theory to account for the results.

Retroactive inhibition.—Newton & Wickens (114) performed a series of retroactive inhibition experiments in which they varied the point of interpolation of the material. Original learning and relearning of the original list were separated by 48 hr. The interpolated list was placed 0, 24 or 48 hr. after original learning. When they used Si-Ri and Si-Rii material, they found no differences between the three positions (there was warm-up learning in the 0 and 24 hr. groups) and thus agreed with Archer & Underwood (5). When they used Si-Ri and Sii-Rii, they found a greater effect of the interpolated learning at 48 hr. than at the other two points. They suggest that Archer & Underwood may have had a decremental effect at 48 hr. which had been hidden by the increment attributable to warm-up [Irion (81)].

Serial learning.—Noble (115) demonstrated that familiarization with the meaningless verbal material of a serial learning task made a significant reduction in the number of trials for mastery of the material, and the degree of facilitation increased curvilinearly with amount of prior experience. Cantor (26) used 3-to 5-year-old children and gave one group practice at naming two

pictures of faces subsequently used in a transfer task. A second group had irrelevant training, and a third was simply exposed to the stimuli. It was found that in a simple discrimination learning situation involving the faces, the group which had acquired names for the faces learned fastest.

Schulz (130) has tested the hypothesis that the serial learning curve is attributable, at least in part, to the necessity of learning both the items and their positions. After learning a list the subjects were asked to indicate the position in the list of each syllable. Position confusion was taken as an operational definition of serial position generalization. Partial confirmation was obtained.

Meaningfulness and isolation.—Kimble & Dufort (84) varied the meaningfulness of the material in a paired-associates task and found to their surprise that the least meaningful material was easiest to learn. They went on to establish that isolation, their explanation of the original results, would be more effective if the isolated term were a stimulus word than if it were a response word. Finally, they varied the relative meaningfulness of a word in the middle of the serial position curve and found that isolation produced a clear dip in the curve.

Number of competing associations.—Beecroft (10) had subjects learn paired adjective lists which varied in terms of the number of competing associations. He found that competing associations handicap performance early in learning and that intralist similarity did not affect recall.

Implicit verbal chaining.—Russell & Storms (128) seem to have demonstrated facilitation of learning with implicit verbal chaining with more than one link. They used the Kent-Rosanoff association test words to elicit associations and thus establish the existence of chained associations from B to C to D. They then gave A-B training. Finally, they gave A-D training matched in difficulty with A-X training and found superior A-D performance.

Response generalization.—Young developed a series of predictions based on response generalization (161). He predicted that in learning successive paired associates lists, the strength of association in the first list learned would increase as response similarity increased. This should result in decreasing retroactive inhibition and increasing proactive inhibition. He found that positive transfer increased with response similarity and that retroactive inhibition decreased, but he found no difference in proactive inhibition.

Generalization of effect.—Marx & Bernstein (98, 99) reported two tests of the Marx & Bunch (100) generalization hypothesis. In one they found that when a response to one of four synonyms in a list was rewarded, there occurred a strengthening of the nonrewarded responses made to the three other synonyms. The second study found positive support for the same hypothesis stated in terms of generalization along the dimension of stimulus similarity.

Motivation and verbal learning.—Farber in a review has brought some order into the field of motivation and verbal learning (48) and in the process rejects many of the studies in this area as having used measures and variables which have not been demonstrated to have had any motivational

properties at all. There is a study which fits Farber's criteria which yielded negative or doubtful results. Kott (89) used sexual, neutral, and unpleasant words in anxious and nonanxious subjects but could not demonstrate results in learning, recall, or relearning scores. Beam (9) measured GSR and learning of serial nonsense syllables under real-life stress conditions such as doctoral examinations, giving an oral report, or taking a role in a play. He found a negative relation between amount of sweat and speed of learning both between conditions and within. He found conditioning rate facilitated in the same situations. Bourne (16) induced tension during learning, recall, both or neither and reports clear evidence that tension facilitates response elicitation but does not alter habit strength.

SOCIAL LEARNING

There is a considerable number of studies in which either a social stimulus is used to reinforce a bit of behavior, or a class of social behavior is itself modified. Studies by Greenspoon (65), Philbrick & Postman (119), Taffel (143), Weiss (157), and Verplanck (151, 152) all use some social stimulus like "good," "mmm-hmm," or "huh-uh," or simply rephrase the subject's response to modify the frequency of either verbal or motor behavior without the subject's being able to verbalize what is happening. Sidowsky, Wyckoff & Tabory (137) seem to have demonstrated some learning in a minimal social situation in which there was a degree of interdependence between two subjects working at the same time without the knowledge of the presence of the other. Azrin & Lindsley (7) demonstrated acquisition, maintenance, and extinction of operant behavior in children where reward was contingent upon co-operation between two children who could not see each other. Miller, Murphy & Mirsky (102, 113) were able to produce a considerable but not simple modification in the dominance hierarchy of 10 rhesus monkeys by making the eighth in order the conditioned stimulus for pain avoidance.

Davitz & Mason (32) set up a conditioned fear reaction in rats by pairing a blinking light and shock. They then used an activity measure of fear and found that the presence of another animal could attenuate the fear response.

CONDITIONING

Acquisition.—Fitzwater & Thrush (50) report a study on CS-US interval in classical conditioning. They find little learning at intervals of .0 or .1 sec., a curve rising to 20 to 28 per cent CR's through .2, .3, and .4 sec. and falling to 12 per cent at .6 sec. Razran (126), however, concludes that backward conditioning has been shown to occur at 2 sec., is better at 5 sec., and best at 15 sec. with food as the US. He opines that backward conditioning can occur with shock only when the CS appears after the termination of shock. Brush, Brush & Solomon (20) in avoidance conditioning with dogs compare trace and delayed conditioning and length of CS-US interval (from 2.5 to 80 sec.). The longer intervals give slower learning and higher asymptotic latencies. In comparing their delayed procedure with Kamin's (82) trace procedure,

they find that delayed conditioning produces faster learning and higher resistance to extinction. Kimble, Mann & Dufort (85) have compared classical and instrumental eyelid conditioning and found faster conditioning with the classical procedure. They go on in a series of experiments to the conclusion that learning occurs in a few trials in the sense of CS-US pairing (the CS can be omitted on many trials without decrement), that the US acquires additional, learned motivational properties in a series of trials, and that this increment shows in performance. In classical conditioning it is the performance rather than the learning that is at a higher level.

Barnes (8) has found faster conditioning with more intense CS in contrast with previous findings. He used a pure tone as CS and shock to the paw of a dog as a US. He also varied CS-US relations in the following manner. The onset of the tone was always followed .9 sec. later by a .1 sec. shock. In different groups the tone was terminated at 0, 5, 15, and 30 sec. after shock termination. In general he found that the longer the tone termination was delayed, the smaller the number of CR's in a standard series of trials. In contrast to the findings of Kimble, Mann & Dufort, it appears that in these groups the CS might be acquiring differential motivating properties and thus be affecting performance.

Kimble (83) also reports a study in which shock intensity was varied where he found response latency decreased with increases in shock strength. Since he found no differences in extinction between the groups, it would appear that in this study also shock intensity had influenced performance rather than learning. In the light of this study a research by Beam (9), in which conditioning rate and level were both influenced by the amount of stress in conditioning GSR to stress in real life situations (before doctor's orals, etc.), would seem to be evidence of the effect of stress on performance. Taylor (144) adapted groups differentially to the air puff in eyelid conditioning. When subsequently conditioned, the unadapted group gave the greatest number of CR's.

Razran (122) has pointed out a series of differences between classical conditioning and operant conditioning, and has performed an experiment in which classical salivary conditioning in humans was carried on during training on two associative learning tasks (123). He found that conditioning proceeded best when the subjects did not know they were being conditioned and that associative learning was reasonably effective when the subjects knew what they were associating. He has concluded that Pavlov's laws of conditioning are primarily laws of unconscious biological learning and that learning at conscious levels may well follow principles quite different from those found by Pavlov. In a further article (124) he identifies second order conditioning with secondary reinforcement and says that both require some sort of organismic cognition or verbalization to become a part of man's permanent behavior repertoire.

Dykman & Shurrager (41) report spinal conditioning in one puppy and 12

of 15 kittens. They argue that these data are not subject to the criticism previously applied to spinal conditioning data.

Extinction.—There are a host of studies of factors influencing resistance to extinction, but hardly two investigating the same factor. Lewis (92) has reported a big factorial study in which he varied the per cent of reinforcement, the spacing of acquisition trials, the spacing of extinction trials, and the period between acquisition and extinction. He reports a large number of results worth further study. Among those of unusual interest are: (a) that spontaneous recovery was significantly greater following a massed acquisition interval than a spaced interval (which supports the notion that the greater the effort the greater the learning); (b) evidence that there is a frustration drive present during extinction, especially during massed training; and (c) a failure to find a significant interaction between per cent of reinforcement and acquisition interval (which argues against the explanation of the efficacy of partial reinforcement in extinction as being a result of reinforcement in the presence of the cues of nonreinforcement). Wilson, Weiss & Amsel (159) also failed to find support for this contention.

Moltz (103) compared latent extinction with the food cup present and absent during the latent extinction period and found evidence that it is the extinction of the secondary reward value of the food cup which accounts for the effect. In all groups extinction was much faster with the food cup absent during regular extinction. Adelman & Maatsch (1) varied the reaction permitted to the frustration of finding no food during extinction. If animals were permitted to recoil from the food box (an act incompatible with the approach response), they extinguished faster than animals simply confined to the goal box, and animals permitted to climb out of the box did not seem to extinguish at all. Their results are in conformity to elicitation theory.

Mackintosh (95, 96) varied reinforcement schedule during learning but interprets it as representing irregularity. She introduced two other forms of irregularity: the number of manipulanda (one to three) and the number of drives reduced, hunger and thirst. Different numbers of manipulanda varied the resistance to extinction while different numbers of drive did not.

Wickens & Snide (158) report what seems to the present reviewer to be an interesting finding in a similar vein. They conditioned three groups to a complex CS of a light and tone, with a shock as US and CSR as the response. One experimental group had the tone omitted on a few acquisition trials, and the other had the light omitted. Both experimental groups showed greater resistance to extinction than the control group conditioned to the complex on every trial.

Sidman (135) has demonstrated avoidance behavior with high resistance to extinction, and Hall (67) has provided some rules for therapy. She has shown that the passage of time is no help and that Pavlovian extinction will work eventually but will be slowed by blocking the escape response and hastened by providing the comforting presence of another animal.

Razran (125) offers a two-stage, two process theory of extinction. There is an early stage of automatic deconditioning attributable to loss of proprioceptive and interoceptive feedback CS's when CR is reduced, and a later stage of counterconditioning.

POSTSCRIPT

No one can complete such a review without major concerns. Lack of space has required the cutting out of whole subject-matter sections. Lack of time has enforced a restriction in the readily available journals covered. Lack of availability has eliminated many good articles in widely scattered sources so that the presence of a few such is almost adventitious. There is a large foreign literature, mainly Japanese and Russian, not covered for lack of language facility. Within the literature covered there has been, of necessity, a simplification of the fine and subtle points which are the sinew of a growing science.

If any large trends are detectable, they would be the finer and finer differentiating of the subject matter falling under the rubric of learning and a gradual merger of "learning theories" with theories of perception, neurophysiology, motivation, and personality into "behavior theories." One might predict that rather than a third edition of *Theories of Learning* (72), Hilgard might well write a book 10 years hence titled *Theories of Behavior*. We shall see.

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DEVELOPMENTAL PSYCHOLOGY^{1,2}

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INTRODUCTION

For the first time in its existence, the *Annual Review of Psychology* includes a chapter entitled "Developmental Psychology." It is gratifying to see that this is not only a matter of form, but a desire to bring child psychology out of its current isolation in order to reintegrate it into general psychology.

The pioneers in child psychology—one need but recall the remarkable work of a Baldwin (12) or a Claparède (39), a Bühler (31) or a Koffka (106)—established a close relation between the study of the child and that of the general laws of adaptation and mental evolution. In fact, child psychology was in its beginning as much a method of investigation as an object of study. This outlook has remained particularly alive at Clark University and the University of Geneva. Werner (198, 199) continues to point out that "developmental conceptualization is applicable to the various areas of life science and is potentially useful in interrelating the many fields of psychology. General psychology ought to be fertilized by child psychology, i.e., by injecting into it developmental methodology."

Piaget (140, 141) has developed a theory of intelligence, a theory of perception, and also one of genetic³ epistemology; in this last, he has fused his research on the development of knowledge in the child with a historical-critical study of scientific thought.

In the field of French psychology, Wallon (192, 193), through his clinical observation of the abnormal child, has been the main contributor to the theory of emotion.

However, a review of the last few decades shows that such efforts have remained isolated. On the whole, child psychology in achieving its individuality has lost in theoretical conceptualization. Dennis (51) views the current situation as quite inconsistent: "We support science to an extent to which it has never been supported before. At the same time, there is much public concern with problems of child rearing, personality development and education. But a genuine experimental attack upon the problems in these fields is almost lacking." This state of affairs seems to be the result of several causes, the most obvious being the distance which often separates child study centers, created for the most part for welfare purposes, from the laboratories of experimental psychology. Moreover, scientific progress has

¹ The survey of the literature pertaining to this review was completed in May, 1956.

² Acknowledgement is made to the librarians Liliane Coutau and Louise Roch for their valuable help and to Mrs. Hannah Niedorf who kindly translated this review with the assistance of Dr. Melvin Weiner.

³ The word genetic is used in the following sense: the origin and historic unfolding of mental life.

placed before the psychologist such a complexity of interrelated facts which do not lend themselves to reduction into simple factors or equations, that the result has been a certain lack of order and the need to revert to new models and instruments of analysis. Without a doubt, there has been no lack of fruitful hypotheses. These were for the most part borrowed from clinical and animal psychology. However, the excessive concern of certain schools of psychology to preserve their achievements has resulted in the limiting of their hypotheses and their methodology, and to that extent this has somewhat held back scientific progress.

In the preceding issues of the *Annual Review of Psychology*, and with special emphasis in the last volume (5), one sensed the desire to attempt an integration of our knowledge to date into a general theory of behavior and development. Consequently, the extent to which this desire has found expression in the year's research will serve as the guiding theme throughout this review and the criterion for the choice of the works mentioned.

The honor of reviewing the year's work in child psychology has fallen to a European. This choice would seem to indicate the wish of our American colleagues to renew and intensify scientific contacts with the Old World. It is more for this reason than for subjective reasons that I have concentrated on work with which I have direct acquaintance. On the other hand, this account will suffer numerous omissions, firstly those of documentation. Although an attempt has been made to give a truly international survey, certain research, particularly that of the Latin American and Asian countries, has not been obtained within the limits of time imposed by publication. Secondly, there come omissions attributable to one's outlook, for in spite of the universality of observational and experimental methods, the attitudes influencing much psychological research are often, consciously or not, of a cultural or epistemological nature. For example, the interest shown for the role of language in mental activity as a "secondary system of signalization," that shown for tension reduction and social adjustment, and finally that for the laws inherent in the development of reason cannot be fully appreciated without a deep knowledge of their cultural and historical background.

Needless to say, this review has been limited to studies concerning the mental development of the child, leaving to others the discussion of the pathological, therapeutic, and educational aspects of child psychology, as well as that of assessment techniques.

If this fragmentary and somewhat one-sided summary will nevertheless prove to be a useful source of information, this will be a result in part of the friendly collaboration of my colleagues. I sincerely hope that this collaboration of an international character will grow into a truly scientific exchange.

SENSORI-MOTOR FUNCTIONS AND LEARNING PROCESSES

It would be artificial to attempt to describe separately the research carried out on motor functions, perception, problem solving, and their learning. Perception and movement are considered as being integrally related. In the opinion of some authors, it is not only the large movements of

the eye but micronystagmus which is involved in visual perception. Thus, the first manifestations of problem-solving are founded on the interrelation of perception and movement. In certain laboratories, a systematic study has been made of the age factor as a variable in development. In others, particular attention has been paid to the effects of various determinants, such as the emotional value of stimuli and the role of language. Also, important differentiations attributable to sex differences and to social conditions have been observed.

It is known that the Soviet psychologists [e.g., Sokoloff (171)] attribute the fundamental perceptual mechanisms (e.g., size constancy) to neurodynamic regulation. The stimulation of the proprioceptors of the eye muscles would contribute to the impression of distance, whereas the modifications of the retinal image would be responsible for apparent size perception. Thus size constancy would be a result of generalized responses of reflex mechanisms to complex stimuli. Klix (103) who demonstrates a certain attachment to Gestalt psychology believes size constancy to be attributable to regulator mechanisms of a phylogenetically determined nature. In both cases, the regulations at work are not fixed but are conditioned by the current or habitual relations which the subject establishes in his perception of objects. A genetic study has been made of determinants such as the functional role of the stimuli and of language as a "second system of signalization."

Luria (117, 118) studied a set of twins aged five and one-half years and observed a considerable retardation of language exclusively attributable to the absence of a need for verbal communication. In this case, other forms of communication had been sufficiently developed. When the twins were separated, a profound repercussion on the symbolic and cognitive functions was brought about by the exercise of language. He also stressed the generalizing function of speech which gives stability to the child's notions of time. Endovitskaia (57, 58) made a study of the variations in visual thresholds in children four to seven years old. The threshold varied according to recompenses offered and according to the awareness brought about by verbalization of spatial relationships. She also demonstrated the role of language as an instrument of conceptualization in the classification of objects. Elkonina (56) showed that the processes of excitation and inhibition developed in the first system of signalization are directly transmitted to the second system and vice-versa. The differentiation of these processes is achieved only very gradually. Zarozets (201) observed the effect of visual exploration and later that of language on the learning of intentional movements.

These results seem to differ somewhat from those of certain American authors. Smith & Goss (168) in order to test the hypotheses of Miller and Dollard made a study of the learning of motor tasks in children of four and one-half to five and one-half years of age. While leaving open the question of whether a sufficient command of language was present, they did not find any significant facilitation attributable to verbal discrimination of the stimuli. However, Cantor (33) in studying three- to five-year-old children was able to show that in a transfer situation involving the choice of drawings

of faces, the possession of names for the stimuli in a learning task enhanced performance. Dietze (52) likewise pointed out that four- and five-year-old children learned form discrimination more rapidly when the names of the forms themselves were very dissimilar. Hunter (89) also investigated the discrimination of two differential stimulus components, such as color and form, in a transfer situation. He showed that the effect of training augments with age and that the transfer is a result of better perceptual articulation. Spiker & Terrell (175), using a three stimulus discrimination problem with three to seven year old children, showed that specific training increased the frequency of correct responses in children who had previous general training; the effect of this training was more marked if the children knew the concept name. In addition, the authors observed that the majority of the children did not know an appropriate term for the concept of middle-sizedness. Two studies were undertaken, one by Fattu, Auble & Mech (63) and the other by Auble, Hurst & Mech (7), in order to measure the effect of partial reinforcement during the period of extinction. Auble & Mech (8) found that a generalizing effect exists between the reinforced subjects and the subjects that are simply in the presence of the reinforcement. On the other hand, there appears to be an increased variability in performance when verbal reinforcement is withheld after being administered continuously.

The preceding review of studies carried out on the various aspects of sensori-motor functions shows us what yet remains to be achieved. However, one must guard against the dangers of oversimplification in the interpretation of data of this kind. A tendency towards this is shown in the work of Château (36) who attributes variations in perceptual judgment to an all-inclusive emotional factor. However, the excellent research of Witkin and his co-workers (200), the outcome of 10 years' team-work, demonstrates the value which can be derived from the coordination of experimental studies of perception and clinical investigations of personality, using the same subjects. It would therefore seem important to study the sensori-motor functions in learning situations, and especially to observe sensori-motor functions in their natural context, for the purpose of re-examining from a genetic viewpoint the principles described by Bruner & Postman (29).

The work of Piaget and his collaborators is a good example of the possibility of using child psychology and the genetic method as an instrument for solving certain general problems in psychology.

In the field of perception, Piaget, while retaining from Gestalt theory the concepts of wholes and of equilibrium, attempts to study perceptual phenomena from a functional and probabilistic point of view. He considers it essential to have some knowledge of the evolution of perceptions in the child and adolescent as well as those of the adult, in order to be able to demonstrate the development of perceptual structures with age.

In reference to the two classical types of perceptual errors or illusions, he found thus that the first decreases and the second increases quantitatively with age. In addition, he found a third type of error which increases up to a certain age (usually up to 9 to 11 years) and then decreases. Piaget con-

siders the perceptual effects of the first type of error (an illusion decreasing with age) as primary effects depending on field effects, the field being defined as the immediate interaction of simultaneously perceived events. The second and third types of error, on the contrary, are interpreted as secondary effects resulting from perceptual activities. These activities consist in exploration of the figure, comparisons in space and time, forming relationships to systems of reference, transpositions, anticipations, etc. These activities as such generally lead to a decrease in perceptual errors. But they sometimes lead to contrast effects, which in turn lead to an increase of the illusion even in older children.

Over a number of years, Piaget and Lambercier have systematically studied most primary illusions and a large number of secondary effects attributable to perceptual activities, as a function of age. Primary illusions were investigated in children from 5 to 12 years of age. The figures studied were the Delboeuf, Oppel-Kundt, Müller-Lyer illusions, the illusions of the angle, the rectangle, and the trapezium. Not only were these figures studied in the classical way, but they were systematically varied with respect to the relative size of various elements of the figures in order to determine for which proportions of the figures the illusion was maximum and minimum. It was found that the positive and negative maxima are the same at all ages, and only the absolute value of the illusion tended to decrease gradually with age.

An attempt was made to find a general explanation for these curves of errors, which are qualitatively identical at all ages. This explanation was derived from the general fact that elements which are fixated are overestimated at the moment of fixation. Piaget (148) has recently elaborated a probabilistic interpretation of the phenomenon of overestimation by centralizations. This interpretation is based on a schema of probabilities of "encounter" and has given rise to a general law for optico-geometric illusions, i.e., the law of relative centralizations. Piaget & Morf (145) checked the effects of overestimation as a result of centralizations in the adult and in the child and found that these effects are more marked in younger children than in older ones.

Piaget & Pène (150) applied the law of relative centralizations to the illusion of angles. Piaget & Denis-Prinzhorn (143) applied it to the illusion of partially superposed quadrilaterals. In both cases, the illusion decreases with age, except when there is relating to a frame of reference. Piaget & Morf (149) made an attempt to reduce the illusion of acute and obtuse angles to a single effect resulting from the slope of straight lines, an effect which also can be explained by the law of relative centralizations.

Piaget, Privat & Maire (144) and Piaget & Stettler-Albertini (146) showed that the perception of "good Gestalten" undergoes an evolution with age. Gestalten based on the primary effects of perception are much less resistant to distortion in the child than in the adult. The distortion of a square with superimposed Müller-Lyer arrows is three times greater in the child. But on the other hand, when a Gestalt is perceived as a result of

perceptual activity, consisting of systematic exploration and comparison of the sides and angles, it acquires a greater resistance to distortion. Whereas early perception is largely influenced by the field effects, later these effects lose their potency as a result of perceptual activity.

Two studies inspired by the observations of Tinbergen (187) and Lorenz (115) were undertaken on early perceptions of human beings. Gunther (75) considers the nipple-shape and the protractile tissues as a sign-stimulus working on an innate release mechanism. Ahrens (2) investigated changes in meaning of the infant's smile. He considers the first smile as the manifestation of an instinctual mechanism, since the smile seems to be released without any apparent stimulation. The smile of recognition which appears in response to human faces, would seem to be subject to a law of the summation of stimuli and not to an immediate Gestalt.

Among experiments based more or less directly on Gestalt psychology, we may mention those of Gottschaldt and Fraisse. The former (74) repeated Köhler's (105) famous experiments from a genetic point of view. He observed how normal and abnormal children ranging between the ages of three and seven years discover the construction and utilization of instruments through a park fence. According to his findings, the solution of the problems would depend not only on the dynamic peculiarities of the topological field and the internal dynamics of the subject, but would depend mainly on the instrumental significance attached to the objects. Fraisse *et al.* (68, 69) in two preliminary studies to his research on time perception found that spontaneous motor time (tapping rhythm with a Morse key) increases between the ages of five to seven and reaches a plateau towards the eleventh and twelfth years. The dispersion of the results, which increase with age, is minimal during the period of the most rapid spontaneous time, that is towards seven years old. In addition, it has been shown that the evaluation of time in situations of expectancy is in part influenced by the emotional stability of the subject. Vernon (191), following in the tradition of Helmholtz, stressed the role of perceptual schemata. This persistent, deep-rooted, and well-organized classification of ways of perceiving is based upon individual knowledge of what sort of thing to do in, and about, certain kinds of situations. It would appear that the child, as a result of his own manipulations, becomes aware at a relatively young age of the fact that objects maintain their identity, their real shape and size, even though their aspect changes with respect to their position in space. Hunton (90) studied the recognition of inverted figures in young children. Although this recognition becomes more accurate with age, it still depends on absolute spatial orientation (i.e., implicit systems of coordinates which are acquired through a process of generalization). Castaneda & Palermo (35), using an experiment in the relearning of psychomotor tasks, analyzed the role of training and stress in performance. It was assumed that stress, under the conditions of this study, affected the level of drive. They demonstrated that the degree of difficulty or facility in relearning a task was a function of the relative strength of the dominant habit. Rey (155) devised an experiment for the

localization of a visual stimulus in the form of a dot on a sheet of paper. He was able to show considerable improvement in localization between the ages of 5 and 12 years, depending on the extent to which the subject was able to fixate on the stimulus and to localize it in relation to the marginal frame of reference. Ohwaki (138) has pointed out that Charpentier's weight illusion increases rapidly between the mental ages of four and five years, since it depends on the differentiation and integration of the different sensory fields.

Among the investigations devoted more specifically to motor performance, we should like to mention the work of Ammons, Alprin & Ammons (3) who observe an increase in proficiency accompanying an increase in age in continuous rotary pursuit performance. Also the work of Sloan (167) may be noted; he constructed and standardized a motor developmental scale using as a point of departure the Oseretzky test of motor proficiency. Rey (154) and Richelle (158) designed an experiment to study the voluntary control of graphic movement. The task consists in drawing a line of 50 cm. in length as slowly as possible over a period of two minutes. A comparison was made of two groups of children, the first, a group of young Moroccans between the ages of 11 and 16, and the second, a group of Genevese children 5 or 6 years younger. The ability of the first group was less than that of the second group, and this difference was greater for this test than that for intelligence or even psycho-motor tests.

It is worth mentioning two publications on research done on the body image. The first by Benton (17) is based on experiments of finger localization in normal children and in children suffering from brain injuries. The second, by Langeveld (110) deals with a few observations accompanied by existentialist reflections. In addition there is the work of Taylor & Thompson (184) on the preferences for certain facial proportions in children and college students. The theoretical significance of this study is not immediately apparent.

The problems of sensori-motor learning have been studied with much success in young animals. Adler (1) ingeniously showed that observational learning in young rats (6 to 12 months old) is particularly effective during the first trials. The rat seemed to observe primarily the objects as they were moved by the experimenter rather than the position of the object in space. Fuller (71) subjected 19 week-old purebred dogs to the learning of highly specific items. He concluded from the weak correlation ($r = .18$) between the tests that there was a genetic influence on the organization of the specific activities.

Drever (54, 55), basing his work on that of Hebb (82), has studied spatial learning in blind children. Drever's work and the above-mentioned studies all point to the need for learning theory to take into account the distinction between early and late learning. Also, greater use should be made than heretofore of the genetic approach in order to clarify learning theory itself.

While most developmental studies are carried out with children, two researches by Weiner (196, 197) demonstrate the effectiveness of the developmental approach with adults. In the first, subjects developed a new

organization of postural and visual cues in perceiving the upright. The second is a very detailed account of the processes involved in the development of new percepts in a distorted room. In this study there is presented an interesting theory of perceptual development which has many striking aspects of similarity with the development of percepts in children.

SYMBOLIC AND COGNITIVE FUNCTIONS

We shall give a brief description of the research which has contributed to the study of symbolic functions as they manifest themselves in play, dreams, drawings, and language. This description will be followed by some indication of the few studies undertaken on the formation of thought itself.

No outstanding research has been done on symbolic play in spite of the fact that, in giving the child the liberty of transcending reality through fiction, this stimulates the full development of the personality [as pointed out by Hetzer (85)]. Moyer & Gilmer (129) determined the attention-span of children between the ages of one and one-half and seven years, for different types of toys. As might be expected, the attention-span not only increases with age, but depends mainly on the choice of adequate toys at each particular age level. The closer a toy comes to satisfying the particular needs of a child, the higher will be the play value of the toy.

Thomae (185), using the time sampling method to observe spontaneous play behavior, systematically compared a group of children aged eight months to two and one-half years with another group of four to six year old children. He demonstrated that the behavior of the younger children was marked by a greater periodicity than that of the older children.

The publications on drawings, while lacking in outstanding contributions, nevertheless have introduced new points of view. Mühle (130) examined the classical interpretation of drawings in the light of the psychology of wholes (*Ganzheitspsychologie*). The Soviet psychologists, as Baumstein-Heissler pointed out (14), consider the child's drawing as an effort to see reality as it is and to reproduce it in as convincing a way as possible for others. We see here the prime importance given to the outer world and to the social factor, both dear to Soviet thought. The individual content of children's drawings has been studied by van Krevelen & Martens-Wartena (108), the self-portrait as a diagnostic tool by Stewart (179), and the esthetic judgments of paintings of different styles and periods by Subes (180). These judgments, in a given culture, seem to change with age. Children of five to seven years old show a definite preference for abstract art, while older children seem to prefer realist art. And lastly, Reynolds & Stacey (157) again pointed out the well-known inferiority of subnormal children in mirror-drawing.

However, it is research on language which has attracted the greatest interest of child psychologists, and also of logicians, cyberneticians, and sociologists. Revesz (156), in a symposium on thinking and speaking, postulates the interdependence and indissolubility of these two functions, whereas Piaget, for example (as also pointed out by Eliasberg), maintains that thought precedes language in the child but that in return the latter pro-

foundly transforms the former. Without the system of symbolic expression constituted by language, operations remain at the stage of successive actions and remain incommunicable. In its double role of symbolic condensation and social adjustment, language is indispensable for the elaboration of thought. Chulliat & Oléron (38) applied the problem-solving tests of Rey (153) to deaf-mute children five to eight years old. They observed that the results of the deaf-mute children were inferior to those of normal children, since the former seemed to have some difficulty in gaining an over-all view of the problems to be solved. Levine (113) studied ways of learning verbal material through repetition in relation to mental development. Roquebrune (160) analyzed children's narratives from a projective test by Murray and observed that among three types of narrative: imaginative, projective and rational, only the first had a distinct and direct relation to age. Moreover, the use of the past tense seemed to give way to the present tense, at least in young Parisian children. Hetzer (84) made an analysis of the style of written compositions of children and adolescents. In a discussion of the effects of bilingualism on scholastic achievement, Soffietti (170) suggested that in studying these effects a distinction should be made between bilingualism and biculturalism.

Among the few studies of the mechanism of memory, we may mention that of Rösler (161) who examined groups of normal and feeble-minded children with the Lewin-Zeigarnik test. He was able to demonstrate important differences as a result of the dynamics of the mnemonic process. Whereas normal children retain unfinished tasks better and, when interrupted, relinquish their activity with greater difficulty, imbeciles seem to retain finished tasks better than goal-directed unfinished tasks. Also they are interrupted more easily in their tasks than normal children, except in the case of repetitive activities which tend toward stereotyped behavior.

At a time when psychology is often considered to be reducible to physiology on the one hand, and to sociology on the other, relatively little attention has been paid to the study of the cognitive functions, and in particular to that of problem-solving and of the formation of concepts. And the "genetic" method, which consists of studying the process of intellectual adaptation in its evolution, is applied systematically in but a few research centers.

Inhelder & Piaget (94) extended their investigations of the child's thought to that of the adolescent. The adolescent's thought seemed to manifest itself in a particularly characteristic manner in inductive reasoning. In fact, the way in which children and adolescents do a physical or chemical experiment and attempt to test the hypotheses set forth is extremely revealing of the level of mental operations involved. Inhelder and her collaborators designed a series of experimental set-ups, each of which is simple enough for five year old children to handle and is interesting enough for 15-year-old adolescents. Thus, they studied the transformation of behavior involved in problem-solving as a function of age. Piaget, on the basis of these results, then submitted the reasoning processes to analysis by sym-

bolic logic. He was able to demonstrate that the inductive reasoning of the adolescent is determined by structures clearly definable by algebraic models, that is by "groups" and "lattices." In effect, whereas the experimental processes of children only have recourse to the so-called concrete operations of classes and relations (seriations, correspondences, etc.), those of adolescents have recourse to the so-called formal operations. The outstanding characteristic of formal operations is their ability to synthesize the concrete operations elaborated during childhood into a total system, and to transcend them on the level of the logic of propositions. Without doubt, such results are only valid for a given environment. However, they open up new horizons in the examination of two aspects of thought processes: first, the continuity of intellectual development in which each structure of thought serves as a point of departure for the attainment of the next, and second, the structures which point to the culmination of the development of operations, i.e., the structures of "double reversibility" (inversion and reciprocity).

Two researches on the formation of probabilistic reasoning, while increasing the complexity of the experimental situations, confirm the results formerly obtained by Piaget & Inhelder (142). Cohen & Hansel (41, 42), who concern themselves with risk-taking problems, analyzed minutely the different kinds of guesses made by preadolescents in experimental situations. These experiments sometimes resulted in a choice between two alternatives and sometimes in a fortuitous distribution. The authors also showed that the idea of the independence of fortuitous phenomena appears from 12 years onwards and is strongly maintained by certain 14 year old subjects. However, even at this latter age, the idea of chance predominates, and some residues of magical conceptions seem to subsist. Crandall, Solomon & Kellaway (44) carried out an experiment consisting of a series of guessing trials in which the subjects had to guess whether or not a given card would appear on top of a shuffled deck of cards. They found that objective probability was a major determinant of expectancy statements. Furthermore, Inhelder (93) has given an initial outline of the research in progress in Geneva on the continuity of the relations existing between perceptual configurations and conceptual operations. In contrast to the opinion of certain authors, the solution of progressive matrix problems is not attributable to one factor alone. It is possible to distinguish the extent to which intellectual operations are involved from that of perceptual structurings, but the balance of these two factors is not the same in all cases. In certain cases perceptual structurings facilitate operations, in others they obstruct them. Therefore, it is interesting to study how logical operations (addition and multiplication of classes), which are essential instruments of conceptual thought, free themselves from perceptual dominance and to see which evolutionary laws they follow. This can be done by various experiments utilizing concrete objects, pictures, geometrical surfaces, etc.

Carpenter (34) in a pilot study tried to quantify some of Piaget's earlier results, unfortunately without giving sufficient indications of the standardiza-

tion used. However, in the area of operational tests it would seem that only ordinal number scales, such as scalograms, are adequate. Carpenter nevertheless was able to demonstrate a correlation of .86 between the results of the tests and mental age, while the correlation with chronological age was .68. Fischer (65) attempted a factor analysis on a small sample of 80 children which were 11 years old. He pointed out, among other things, that two of the tests (measure of geometric volumes and notions of probability) showed considerable resemblance (angle of 20°), were of almost equal intensity, and seemed to be strongly saturated with the *g* factor. Kostiouk (107) resumed the researches on the first notions of number in two to four and one-half year old children. Since he believes that action is the underlying process of thought, he suggested that one should handle objects in a specific manner in order to be able to abstract the qualities of the objects and their spatial configuration.

Among the studies of classification we should like to mention the results obtained by Lovell (116), who found inadequate abstracting in 14 to 20 year old subjects and was unable to decide whether their inferiority was a result of the cultural milieu or of modifications inherent to adolescence. Calvi (32) followed closely the first stages of abstraction in five- to eight-year-old children. Navarra (132) collected information on the development of concepts in the daily life of a small boy; the anecdotal character of his observations is most refreshing.

Some indication thus has been made of a few of the studies dealing with the mechanisms of symbolic and cognitive functions. As Russell (162) points out, children's thinking, since it participates in most of adaptive behavior, cannot be easily isolated from the emotional and social factors of which it is a part.

SOCIAL AND EMOTIONAL FUNCTIONS

It is with growing interest that psychologists throughout the world are studying the integration of the child into adult society. There are two main trends which, while expressing nuances rather than *real* divergences of opinion, manifest themselves in current research. The first of these trends is concerned with the integration of the child in our current society with its established norms and focuses upon the healthy adjustment of the child. The second trend is concerned more specifically with the building of a future society by the adults of tomorrow. In keeping with this latter conception, original behavior of children and adolescents would be considered as constituting the potentialities of a growing personality capable of contributing to a better future. Our current society is rarely homogeneous, since it is made up of the interpenetration and peaceful coexistence of individuals whose mode of life is largely determined by different traditions. Psychologists therefore are greatly preoccupied with the study of the resultant growing prejudices and antisocial tensions in the child. But in addition, there has been a growing realization of the astonishing plasticity of the young individual with regard to the cultural models offered to him. This realization

has come about more rapidly in the Anglo-Saxon countries, to whom we owe the fine research in cultural anthropology, than on the European continent. Meanwhile, the need remains to investigate precisely which are the constant factors of development in the child and which are the factors modified by the social milieu.

We shall mention first some studies concerning spontaneous social interactions, and then we shall examine the role of the milieu in the child's development.

While the original enthusiasm shown toward the sociometric method has been tempered by a more critical attitude, this method is often used as an auxiliary instrument of research, since it facilitates the formulation of hypotheses or the sampling of subjects in view of more general investigation. It is with this in mind that Fleming (67) used sociometrics to study classroom behavior and that Davitz (49) observed that the highest sociometric choices tend to be perceived as more similar to self than are the lowest sociometric choices. Davitz thinks that this fact should be interpreted as being attributable to the need shown by children to resemble those friends whom they esteem highly. Ausubel & Schiff (9) examined high-school juniors not only with respect to their choices within the group, but also to the way in which each one believed himself to be judged by the others. They showed that the ability to perceive one's own sociometric status and the ability to perceive that of others was completely unrelated. Santucci (164) showed that preadolescents are much less absolute in their judgments of friends than are younger children. However, their choices do not seem to correspond to a scale of values or to any similarities in character traits. Stauffer (178) used sociometrics to form groups of five to eight children who were sufficiently sociable to work on building a construction model together. Among the factors favoring social cohesion, the active child plays an important role in taking the initiative more readily than the others. Nevertheless, Stauffer thinks that lasting co-operation among children is possible only as a result of the pressure of factors outside the group. Noelting (135) gives a provisional outline of his research in progress. He retraced the first social interactions of small children gathered in groups of three around a toy. (For example, the three children together have to construct a small carousel of prefabricated parts which can be set in motion easily.) It seems possible to trace the development of this behavior starting from piecemeal, nonintegrated actions, to that of participating in co-operative construction and of taking one's turn. This co-operation was reached towards seven years of age. Boesch (22) believes that, knowing the level of social performance of a child, it is possible to draw some conclusion as to his degree of sociability. The sociograms which he drew up to this effect showed a relative constancy. However, there were variations in the structure of the sociogram depending on the nature of the tasks to be accomplished. The majority of the children who were the best adjusted socially were also the most gifted, and came, for the most part, from a stable home background. Finley (64) measured the social maturity of preadolescents and adolescents with the Social Inventory

Test. Social maturity to the extent to which it is a co-operative attitude depends largely on the life experience of the individual and consequently varies as a function of the environment in which the child developed (rural, city, etc.).

Rommetveit (159) analyzed in a very pertinent manner social norms and roles, defining the social norm as a pressure existing between a norm-sender and a norm-receiver. He determined the relations which may exist between these two terms by means of a symbolism, the precision of which one may admire, but its utility is not readily apparent. However, in a more concrete manner, he investigated the way in which adolescents behave with regard to two groups of norm-senders, peers and parents.

The effect of collective living on the feeding behavior of certain animals tending toward a social hierarchy has already been studied by numerous authors, but their results have given rise to controversy. James & Cannon (97), like others before them, studied variations in the social facilitation of eating behavior in puppies. They found that the puppies ate more in groups than when fed alone and were stimulated to further eating in groups after having shown satiation in individual feeding. However, they observed that, in this study, the hierarchically dominant dogs showed the greatest amount of social facilitation whereas, in a previous study, the facilitation was greatest for the submissive members of the group. Inhelder (95) observed the play behavior of young mammals in captivity. He pointed out that, in a group consisting of *Wanderus* and one green monkey, there existed a certain dominance order in playing with new toys. The play order coincides only partly with the food order or social order. In the social order behavior of the group, toys had a certain function as symbols of power and as means of communication.

While the mechanisms of social interaction in young mammals, including young human beings, are still relatively unexplored, the role of the milieu in the modification of the individual has been widely investigated. At times, however, the conclusions drawn have been somewhat hasty. Brunet (30), to whom we owe the adaptation of developmental scales to French speaking children, compared three groups of children with each other in the course of their development. The groups consisted of children of psychology students, working class children, and adopted children. Brunet observed to what degree general mental development is favored by the educational climate. Thus, the first group was distinctly favored whereas the last group lacked at least two essential stimulants: affection and maternal pride. However, she stresses that an adoptive home which is lasting and offers good conditions may allow the child to regain his level of development which has been momentarily lowered.

Beizmann (15) observed an intellectual and emotional inferiority in institutionalized deaf-mute children and adolescents, and attributed this inferiority to the deficiency of emotional stimuli and to the small number of models with which the children could identify. Lyon & Vinacke (120) used the Rosenzweig test with two groups of institutionalized and non-

institutionalized Hawaiian boys between the ages of 10 and 13 years. They observed that the expected increase in aggressive responses noted in Mainland children did not occur and could not be attributable, therefore, to institutionalization. The differences in aggressive responses between Mainland and Hawaiian children might be attributed to the reluctant acceptance by the latter of the norms of their surrounding culture. Thus, under stress they would tend to revert to their spontaneous behavior patterns which are less punitive and ego-defensive than those of Mainland children.

Three English studies give evidence of the preoccupation with the effects of socioeconomic background on scholastic achievement. Himmelweit (87) pointed out that middle class boys had higher social aspirations and were better integrated in their school environment than lower class boys. Collins (43), whose interviews seem somewhat superficial, examined the social causes of premature withdrawal from grammar schools. Kemp (101) observed that the level of success in the final junior year was essentially but not solely a result of the intellectual level of adolescents. It was also attributable to their social maturity which appeared to be closely related to their cultural and socioeconomic status. McKee & Leader (122) believe that competitive behavior appears earlier and more intensively in three to four year old children from a low socioeconomic background than in other children.

Without a doubt, child psychology has made important progress in placing the child in his familial and social context in which economic considerations continue to play an undeniable role. However, it is somewhat disturbing to see how easily certain authors establish a cause and effect relationship between the scholastic performance or the emotional attitudes of children and the socioeconomic status of their milieu. And this is especially so, knowing that it is practically impossible to form groups of children characterized by one factor alone and that the plasticity of children with regard to this factor is infinitely variable. In addition, we should like to point out the work of Haesler (76) on children living on the margin of society since they have no permanent home and follow their parents around in their migratory occupation. Also, mention should be made of some very instructive observations by Gerfeldt (72) of the sociopathological causes characteristic of juvenile criminality in our current times.

Some studies have been made of national and racial prejudices, published in a book by Clark (40) and in two articles, one by Fishman (66) and the other by Zelig (202). These seem to indicate that the prejudices of children, while they are obviously a reflection of those of the adults surrounding them, tend either to be reinforced or overcome by direct contacts more than by well-meaning education.

Some important ecological studies have been undertaken which merit our attention. The most typical seem to be those of Barker & Wright (13) and the field studies by Havighurst (79) and Mead & Wolfenstein (123). The first is as vast and complete a description as is possible of the minute-by-minute behavior and of the surroundings of the children in a small

American town. It includes an inventory of the social components of 10,000 episodes in the life of eight children. This work is an exhaustive collection of facts of great methodological value but was not set up to test specific hypotheses and therefore presents no conclusions. Havighurst concentrated mainly on the emotional and moral aspects of the behavior of New Zealand children. We are well acquainted with the abundance of information and the often full-flavored observations of Mead and Wolfenstein. Fresh proof of this is given in their last work in which many observations were presented but conclusions were drawn also from documents unsupported by direct observation.

We see that much has been undertaken in social psychology but, as has been said by Child (37), "the existing body of research findings, while impressive as a whole, is not very solid in detail."

European psychologists are still accustomed to the classical distinction made between experimental method and clinical observation. They therefore feel somewhat at a loss when confronted with the numerous studies devoted to the emotional development of the child, since in varying degrees, these studies use both techniques simultaneously. This year, as in preceding years, a series of studies has appeared ranging from the investigation of ego-development to the observation of the educational climate and its effects on personality formation.

Among the first of these, we should like to mention the testing of some psychoanalytic hypotheses by the long-term observation of young children. Hartmann (78), with his habitual perspicuity, defined sublimation as a process of neutralization by which libidinal and aggressive energy is called away from instinctual to noninstinctual modes of behavior. This would give the ego a certain amount of independence with regard to inner and outer pressures. Hartmann thinks that this process of neutralization begins at a very early age since it contributes to the perception of the constancy of objects. Kris (109) observed this same process of neutralization in three to four year old children's paintings. Mittelman (126) made some excellent long-term observations of the motor development of five children from birth to three years of age. He believes it possible to discern a close relationship between ego-development and what he calls, perhaps somewhat summarily, motor drive. Spitz (176, 177), in an outline summarizing his previous observations, stressed the role of what he terms the primal cavity (the mouth) in forming the link between internal and external perceptions. Erikson (60) again pointed out the phenomenon of identity diffusion during adolescence. Lechat (111), whose approach resembles that of Odier (137), traced the development of the feeling of security. Rycroft (163) studied the origin of the need for idealization in the child.

Even those experimental psychologists who are somewhat reluctant to accept the conceptual framework of psychoanalytic theory will surely agree that one can but express satisfaction at seeing this attempt to test hypotheses of a clinical origin by precise observations.

Starting from different hypotheses, Meili (124) used the longitudinal

method to observe the first forms of anxiety in the child. He attributes these to an emotional block resulting from the impossibility of elaborating certain stimuli. Metzger (125), with an outlook similar to Dembo's, described the psychological tensions which give rise to the period of negativism during the third year of life. This description was primarily intended for the use of educators.

The effects of family relations on the child's personality were analyzed most fully and competently in the last *Annual Review of Psychology*. Therefore, I shall limit myself to an indication of only a few aspects which are characteristic of the vast collection of material of the current year. I should like to apologize to numerous authors for failing to acknowledge their works which nevertheless have contributed, in one way or another, to the elucidation of the emotional problems of the child. In the last few years, a shift in interest seems to have occurred, and this has happened more rapidly in the United States than in Europe. The interest shown for the libidinal tendencies of the child and the corresponding parental reactions [Birch (19); Katcher (100); Rangell (152); Tramer (188, 189); Zulliger (203)], an interest which is still lively in certain research centers, has slowly yielded to an interest in the formation of feelings of independence and responsibility [Beller (16); Ephrussi (59); Harris (77); Heathers (81); Henry (83); Johnson (99)]. Heathers (80), who has a wealth of material at his disposal for longitudinal studies at the Fels Research Institute, observed that success in performance, stimulated at the outset by the approval of others, contributes to a feeling of self-confidence in the child. In return, this self-confidence makes the child more independent and therefore more tolerant to the rejection of others. Between these two extremes, there is a series of studies whose themes are not novel. Several studies have been made of the relations between frustration, hostility, and aggression in normal and delinquent children [Angelino & Shedd (4); Body (21); Crandall (45); Moustakas (128); Otis & McCandless (139); Sontag (172)]. From year to year, a significant increase may be observed in the number of studies on the effects of the child's emotional deprivation or of the frequently unconscious attitudes of parents on his development [Bernstein (18); Brown & Sigel (28); Crandall & Preston (46); Friedemann (70); Highberger (86); Itkin (96); Jayaswal & Stott (98); Newton (134); Sontag, Baker & Nelson (173); Tasch (183)]. Numerous authors [Briggs & Schulz (27); Block (20); Evart-Chmielinski (62); King & Henry (102); Lyle & Levitt (119); Nordland (136); Souerwine (174)] have stressed the harmful consequences of an authoritarian education, which is but one of many educational influences and is tolerated differently according to the age of the child and the cultural traditions of his environment. A study has been made of the position of the child in relation to his siblings, and it was found that in certain cases this may determine his emotional characteristics [Bossard & Boll (23); Koch (104)]. However, adaptation to the family group is but a first step on the road to independence. Entrance into nursery school presents an obstacle to the achievement of this independence in certain children. This

has been studied by psychologists in the United States, France, and the Soviet Union [Levin (112); Lévy-Brühl (114); Gorbacheva (73)], and their results tend to show some consistency. Particular attention has been paid to the emotional problems of adolescence with reference to the biological changes at puberty [More (127); Davidson & Gottlieb (48)]. Also, these problems have been considered in relation to processes of identification with individuals or groups [Crane (47); Debesse (50); Dragounova (53); Sobieva (169)]. The problem of child rearing as a formative factor of personality exceeds the limits of child psychology and is more a preoccupation of the pioneers in pediatrics, such as Senn (166).

We can make no conclusive statement on the basis of the scientific evidence submitted as to whether one should share the optimism of those who consider a harmonious environment as the best guarantee that the child will realize his potentialities to the fullest extent, or whether one should attribute greater importance to the active role of the child in the formation of his personality.

Child psychologists do not always know how to avoid two pitfalls which confront them. The first of these is the setting up of hypotheses which are too general to be tested experimentally. The second is that of tackling problems where the solutions would seem to be apparent at the start (an impression which anyone may tend to obtain in viewing the researches of others!). McCandless & Spiker (121) have expressed a general uneasiness and alarm at the lack of theoretical insight characterizing our discipline. Baldwin (11) suggests a theory of action to orient investigation in personality development. We may agree with them that child psychology will progress to the degree to which it will become more modest and yet more daring, more modest in the delimitation of problems and more daring in the elaboration of fruitful hypotheses.

TEXTBOOKS

Several new texts and revised editions have recently been published. These publications deal with mental development [Baldwin (10); Breckenridge & Vincent (26); Munn (131); Neuhaus (133); Thorpe (186)] or more specifically with one of its stages [Wattenberg (195)] or one of its aspects [Tanner (181)]. The works of Munn and of Baldwin merit special attention, the former for the judicious choice of the subjects treated, and the latter for its original attempt to explain child behavior not only as an adaptation to the present, but also from its genetic aspect.

Among the works intended for parents and educators [Hymes (91); Ilg & Ames (92); Valentine (190)] those of Ilg & Ames and of Valentine stand out for their solid foundations.

OUTLOOK

Much progress has been made recently in the specification of the criteria of development and in the formulation of hypotheses concerning the laws governing mental evolution. It is not by chance that interdisciplinary study

groups have made the most marked contributions. In fact, whenever different specialists, each one setting forth a particular aspect of development, try to communicate to each other the results of their research, they realize the lack of a system of reference. Thus, during the year 1955 to 1956, three groups of researchers from different countries met for the purpose of contributing to the elaboration of systems of reference. The first (182) devoted itself to the psychobiological development of the child, the second (88) more specially to the homologation of the stages of development, and the third (151) is working permanently on the study of logical and stochastic models of genetic behavior.

Longitudinal studies have already shown that it is extremely difficult to find significant correlations between the different aspects of psychological behavior, and between these and the morphological and psychological aspects of growth. It is not clear whether this difficulty arises from the polymorphous nature of development itself or from the criteria chosen for testing.

Again, the relating of qualitative criteria, such as define the stages of development, has met with similar obstacles. In fact, the stages of emotional development set forth by psychoanalysts of different schools [de Saussure (165); Erikson (61); Bowlby (25)] concern themselves with determinative life activities, with crises, etc., whereas the cognitive functions concern themselves with the structures of behavior. By definition, a structure is a system in which the properties of its elements depend wholly or in part on the laws of the total system. Piaget (147) has succeeded in demonstrating two extreme types of structures (with their intermediates); in contrast to the nonadditive, irreversible structures characterizing the perceptual Gestalts, are the additive, reversible structures (algebraic groups and lattices) characterizing the higher levels of intelligence (with intermediate structures during the pre-operative stages). It is only in this latter type of structure that one clearly distinguishes stages as levels of a formative process which reach their culmination in a specific structure. The development of intelligence would thus be comparable to a process of integration in which the structures develop in a constant order of succession. Naturalistic observation of general stages therefore remains a method too vague for current scientific investigation. Meanwhile, it is of great interest to attempt the transposition of patterns of behavior from one area of development to those of another area. Such attempts have been outlined by Walter (194) who tries to apply the stages set forth by Erikson to cybernetic models, by Anthony (6) who reviews the genetic criteria used by Piaget from a psychoanalytic standpoint, and by Bowlby (24) who observes the first human interactions from the Lorenz-Tinbergen ethologic point of view.

In the explanation of the mechanisms of development, it is striking to note the convergence of opinion and the abandoning of opposition between differing schools of thought. The interdependence of three factors: maturation, experience through exercise, and experience through social interaction, seems to be recognized by all. These three factors interact closely during the development of the child. The problems to be resolved in the

future will therefore be the following: (a) How do these three factors reach an equilibrium among themselves? (b) Does each of these factors, while being a regulating principle of development, obey laws common to all three? (c) And would not such general laws constitute a fourth fundamental factor which would in turn be composed of the laws of equilibration?

It is this last hypothesis that Piaget (151) uses in his current research, for the following reasons: first, for its generalized character and second, for the precision of the probabilistic models involved. In effect, supposing that biological growth adheres to the second law of thermodynamics, this growth of entropy would not be acquired by either physical or social experience but would constitute the product of an independent, probabilistic mechanism which would apply to everything, innate and acquired. Likewise, the structures of thought in the child are not innate inasmuch as they are slow to appear and present variations in the average age of appearance depending on the cultural milieu. On the other hand, structures of thought are not derived from objects themselves but are the necessary condition for apprehending experience. They are not acquired through social experience alone, since they are based upon sensorimotor development and result from the coordination of actions before being related to language. Therefore these operations have but one meaning: they are forms of equilibrium based on the coordination of actions. In short, it is hoped that with the aid of probabilistic models, borrowed mainly from the theory of games, we can one day explain the transition from simple behavior to its highest level of development.

We see then, as was pointed out in the introduction to this chapter, that child psychology is on its way to being incorporated into a more general psychology, so that the genetic point of view becomes indispensable in resolving the major problems before us. And it is only through the study of development that these problems will be seen in their true perspective.

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EDUCATIONAL PSYCHOLOGY¹

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INTRODUCTION

The definition of educational psychology as a field has been of concern to several of the previous writers in this review series. The fact that learning has consistently been the central topic in these reviews supports the use of Carter's (26) definition of educational psychology ("that branch of psychology which deals with school learning and its correlates") as the chief basis for selecting the material to be included in the present summary. With a fairly liberal interpretation of "school," this definition seemed a workable one.

Tyler's (124) plan of organization about three major topics that relate to school learning (the learners, the learning-teaching process, and the teachers) has been followed again in this review. It differs, though, in the incorporation of measurement references along with the topics to which they apply. In common with other reviews, certain topics have been omitted because of their more intensive treatment in related chapters.

The period covered is April, 1955 through April, 1956. A complete survey was made of all articles published in the *Journal of Educational Research*, *Educational and Psychological Measurement*, and the *Journal of Educational Psychology* (the latter publication only through November, 1955 because of an unusual current lag in publication). These three journals had been found to include more than one-third of all articles cited in previous reviews of educational psychology. In addition *Education Index*, *Psychological Abstracts*, and the *Review of Educational Research* have furnished references to relevant books and articles published elsewhere.

Research reports have been given preference throughout this review, although a few articles of a theoretical nature have been included. Selected references have been made to recent research summaries, especially in areas where the volume of published research is heavy. Some attempt has been made to present, at least within the limits of selected research areas, a fairly broad-scale viewing of educational psychology at this time. Such a goal has been sought despite the complex nature of this field which makes any such attempt rather futile.

LEARNER CHARACTERISTICS

The customary classification of learner characteristics studies according to their emphasis upon developmental and individual differences approaches to intelligence, personality, interest, or social characteristics has been followed

¹ The following abbreviations are used in this chapter: ACE (American Council on Education Psychological Examination); MTAI (Minnesota Teacher Attitude Inventory); SVIB (Strong Vocational Interest Blank).

in this section. General works in the area of individual development include Baldwin's *Behavior and Development in Childhood* (9) and Wattenberg's *The Adolescent Years* (127). The differential approach to the study of the individual is represented by a second edition of Tyler's *A Psychology of Human Differences* (125).

Mental development.—A comprehensive review of recent literature on mental development was reported by Pinneau & Jones (98). They noted that several longitudinal studies now provide data on mental development into the adult period. Analyses of such data during 1955 to 1956 include one by Bayley (12), who presents evidence from the Berkeley Growth Study, the Terman Study of Gifted Children, and Owen's Iowa Study that the tested intelligence of superior adults continues to increase after the age of 25. Use of longitudinal rather than cross-sectional data seemed to the author to account for any difference between this and earlier studies.

Socioeconomic status and intelligence.—The development of culture-free tests has long been a goal in intelligence test construction. The extent to which this objective has been achieved has attracted the attention of several investigators. Marquart & Bailey (86) found that, though Scale 1 of the Culture-Free Test (IPAT) was no more culture free than the Stanford-Binet for children from four to eight, Scale 2 of this test appeared more adequate in this regard for children from eight to twelve and unselected adults. Sampling limitations restrict the usefulness of these findings, however.

In separate studies, Diaz (41) and Smith & Caffrey (114) compared mean test score differences on the Davis-Eells Games for Los Angeles area social groups classified by Warner's Index of Status Characteristics; both reports indicate that cultural bias has not been entirely eliminated in this test. Justman & Aranow (74) compared the Davis-Eells Games and the Pintner Intermediate Test as measures of intelligence of poor readers and found little basis for preference.

Academic aptitude.—The large mass of accumulated findings on the relationships between academic aptitude measures and student achievement has been further increased during this review period, but with little improvement in the over-all organization of this area. The limited generalizability of much of this research is to be expected and may, to some extent, be appropriate, but the frequent absence of cross-validation makes questionable even local applications of several of these studies.

At the high school level, the limitations of differential achievement measures as predictors of achievement in various school subjects have been demonstrated. Wolking (129) compared subtest scores on the Primary Mental Abilities (PMA) and Differential Aptitude Test (DAT) batteries and reported moderate to substantial correlations (.47 to .75) between corresponding tests in the two batteries, but higher correlations with grades in high school subjects for the DAT scores. The use of the separate subtest

scores for differential prediction of success in specific high school subjects was not supported.

New developments relating to the American Council on Education Psychological Examination (ACE) and the Ohio State University Psychological Examination (OSU) have followed the announcement that no new forms of the ACE will be published. The Educational Testing Service has released two forms of the high school and college level tests of the Cooperative School and College Ability Tests (SCAT) which provide, as was true of the ACE, verbal, quantitative, and total scores. The development and initial validation of a short form of the OSU has been described by Layton (80).

Comparisons of the SCAT-College level test and the ACE, based on administration of both tests to an independent (private) schools sample, have been reported by Traxler (123) and North (95). No marked differences in difficulty, reliability, or validity as predictors of school success were found between the two tests.

The contribution of speeded tests to an aptitude battery was appraised by Lord (82) by means of a factor analysis of a battery of aptitude measures, varying in speededness, combined with academic grades for an Annapolis student group. The problem of reliability measurement for speeded tests was explored further by Guttman (62). He extended his previous discussion of lower bound formulas and provided a basis for making sharper estimates.

Personality development.—The research emphases indicated by Meredith's (90) review of personality and social development research for the 1952 to 1955 period continued in the period covered by this review. Problems of adolescence, sociometric studies, and the development of techniques for evaluating personal and social development have been stressed.

Two studies reported considerable consistency in personality development from childhood to adolescence. Cattell & Gruen (27) studied the factorial relationship between personality factors of adults and of 11-year-old children. The personality structures were found to be largely the same but with some modifications in factor loadings. Cantoni (23) compared Bell Adjustment Inventory scores for students tested at grade 9, grade 12, and nine years after high school graduation, and found correlations of .51 between the scores for grades 9 and 12, .48 between grade 12 scores and those obtained nine years later, and .35 between the grade 9 scores and the 13-year follow-up testing.

Changes during adolescence.—Psychological changes in adolescent behavior, as indicated by a word association technique, were studied by Powell (99) who prepared a list of words related to seven areas of conflict on the basis of judges' agreement. Reaction times indicated that intensity of conflict in adjustment is at its peak during adolescent years in all areas with possible exception of vocational outlook, and that conflict appeared earlier for females.

Dales (38) developed three scales for measuring progress in develop-

mental task areas selected upon their frequency of mention by early adolescents as problems. These scales relate to interpersonal relationships involved in (a) giving and receiving affection, (b) relating to social groups, and (c) learning one's sex role.

Several studies have utilized essays written by adolescents as a basis for appraising personal development. Strang (119), Havighurst & McDonald (66), and Crane (32) have used this technique to examine social awareness and self-concepts of adolescents.

Social relationships.—A review of sociometric investigations by Mouton, Blake & Fruchter (94) placed special focus on those with behavioral criterion measures. Gronlund (58) found that elementary school pupils' ratings were quite stable over a four month period. In a second study, Gronlund (59) concluded that sociometric status based on any of the general criteria types which he employed provided fairly reliable indices of social acceptability of pupils in a classroom group.

The relationships between positive and negative sociometric ratings and scores on the California Test of Personality were studied by Philips & DeVault (97). They concluded that both types of ratings have distinct meanings and that they should not be combined. The findings of a study by Forlano & Wrightstone (47) of the Revised Ohio Social Acceptance Scale support the validity of this instrument as a group test of social acceptance for junior high school classes.

Prediction of college achievement.—Increasing attention was given during this period to the predictive use of personality and interest measures for college achievement. Scores on aptitude, achievement, interest, and personality tests, administered at college entrance, were compared by Berdie (14) as predictors of degree status and curriculum completed 10 years later. Differential interest tests were found to predict better the educational and vocational patterns of students than differential ability tests. Darley & Hagenah (39) have attempted to high light the problem of reconciling interest theory development and empirical studies of interests. They have presented extensive normative data on the Strong Vocational Interest Blank (SVIB) for college men as well as a selective review of the literature on interest measurement and theory.

Several biographical inventories for use as predictors of college achievement were also developed and reported. Malloy's (85) Life Experience Inventory included four areas: school experience and attitudes toward education, self-appraisal, family relationships, and choice and type of friends. Multiple correlations with grade point averages were significantly increased for both men and women when this instrument was added to a battery comprised of the ACE linguistic section and an English placement test.

A Biographical Inventory for Students (BIS), intended for counseling and clinical work with male students, was developed by Siegel (110, 111). A significant finding of this study was that students who received higher

grades tended to participate in fewer physical, social, and heterosexual activities.

Superior science students.—Strong pressures for the early identification and encouragement of potentially outstanding scientists have been paralleled by the appearance of several studies which direct attention to the distinctive personality and social behavior characteristics of such students. MacCurdy (84) compared questionnaire responses of the winners of the 1952 to 1953 Science Talent Search with those made by a group of college students of similar intelligence, age, sex, and educational level. Differences in personality qualities, attitudes, interests, activities, family history, associates, and science teachers were noted. Brandwein (21) proposed that the making of a successful scientist depends on genetic factors (intelligence and adequate sensory and neuromuscular control), predisposing factors (persistence and questing), and activating factors (opportunity for advanced training and contact with an inspiring teacher).

LEARNING-TEACHING PROCESS

Comments on the centrality of learning in educational psychology have often been accompanied by dissatisfaction with the gulf between learning theory and classroom instruction. Considerable optimism would be needed to find any marked improvement in this situation during the period of this review. However, in his second edition of *Theories of Learning*, Hilgard (68) notes that, despite arguments among learning theorists on their interpretations, many experimental relationships among variables in the learning process are generally accepted. He lists 14 such points on which the majority of learning theorists would agree and makes several suggestions for facilitating the application of learning principles in practical situations.

Classroom learning experiments.—In keeping with one of Hilgard's suggestions, several studies have attempted to validate laboratory principles in classroom situations. Such studies, however, have been generally limited to short-term observations of relatively simple skill learnings, rather than to the more complex types involving reasoning processes.

A series of studies in which various patterns of a verbal reinforcement, such as praise by the teacher for good work, were associated with routine arithmetical skills of grade school pupils has been reported by Mech and his co-workers (7, 8, 88). These studies have confirmed certain findings of infrahuman studies on the effects of massed and partial reinforcement. Some evidence was also found that pupils who were present when other students were praised, but who were without direct personal reinforcement from the teacher, performed as well as the reinforced subjects. These studies suggest the possibility that findings of experimental learning research can be applied to some types of elementary school learning along lines visualized by Skinner (112) in his provocative criticism of classroom teaching.

Comparative studies of teaching methods.—A comparison of the relative effectiveness of democratic, permissive, problem-oriented approaches to

teaching communication skills with a so-called "traditional" approach in which less emphasis is put on student participation, was reported by Jenkins (71). No significant differences were found in communication skills achieved at the end of the instruction, but fewer isolates appeared in the sociometric ratings of the classes taught by the permissive procedures.

Smith (113) studied the effectiveness of the team approach in contrast to the usual lecture-discussion method for teaching general psychology. Although differences in over-all satisfaction were negligible between the two groups, the teamwork group was dissatisfied with the group-incentive system. An upward trend was noted in the relative achievement of the team class on successive tests.

Bond (19) compared the relative success of lectures versus the group discussion-decision method of Lewin in effecting long-term change in health practices, in this case those relating to early detection of cancer. A follow-up, 13 months after the instructional meeting, showed consistently higher performance on three behavioral criteria for the discussion-decision group.

Though valuable in their own specific areas, the contribution of most of the comparative methods studies to the understanding of the learning-teaching process is limited, largely because of flaws in research designs, lack of uniformity in procedure and interpretation, and limitations in measuring instruments.

Individualizing instruction.—Teacher knowledge of pupil characteristics is generally accepted as basic to the individualization of instructional procedures. Certain recent studies have examined aspects of this assumption. Hoyt (69) studied the effect of providing teachers with varying amounts of information about pupil characteristics on achievement of pupils and on teacher-pupil relationships. No differences were found in achievement over a six-month period for three levels of teacher knowledge, but pupils' attitudes toward the teacher were more favorable when teachers knew more about them.

Jarolimek (70) found that fifth-grade teachers were generally informed about a pupil's mental ability, health, and school achievement, but were less likely to have adequate information about his home background and personal-social life. Cronbach (33) presented a carefully developed argument on the dangers of over-differentiation in appraising individuals and concluded that the poorly informed teacher may do better to treat all alike.

Much thoughtful discussion and several reviews of research have been devoted to the various methods of providing instruction for gifted students—acceleration, grouping, and enrichment. The references include that of Worcester (130), the well-planned survey of programs offered by 45 schools or school systems reported by Havighurst, Stivers & De Haan (67), the studies of accelerated college students by Flesher & Pressey (46), and of students taught in special classes for the gifted by Barbe (10).

Teaching skills subjects.—The large number of studies in reading practically requires that this area be handled by references to reviews. Gray (57)

contributed further to his annual summaries of research in this field. He noted that reading research would benefit from better designs but that such improvements would require simple, clear explanations of modern research procedures. Gilbert & Holmes (55) summarized research on the psychology of reading and Sheldon (108) on reading instruction.

The place of phonics in reading instruction was much discussed. The May, 1955 issue of *Education* (15) was devoted to this topic. Carroll (25), among others, compared the research findings referred to in Flesch's controversial *Why Johnny Can't Read* with the actual research reports. The effect of postponement of formal reading instruction until the child is considered ready was studied by Bradley (20).

Russell (104) found that combined scores on auditory and visual discrimination bear a curvilinear relationship to spelling achievement, this relationship being best defined at lower grade levels.

Several studies in the arithmetic area have stressed the meaningfulness of learning experiences. A study of thought processes followed by young children in learning multiplication and division was conducted by Gunderson (61). Riess (102) combined a critical review of the manner in which the fraction concept is introduced in teaching with a historical survey of the approaches used in various cultures.

Concern has been expressed recently regarding the tendency of some students to avoid mathematics courses. The attitudes of students toward early courses in this area consequently seems relevant to understanding such behavior. Dutton (43), using a Thurstone-type scale, found that fairly small proportions of junior high school students, about 14 per cent, expressed extreme dislike for arithmetic. Most students, 87 per cent of them, expressed enjoyment for problem-solving when they knew how to do it well.

Instructional materials.—In the first issue of the *Review of Educational Research* that was devoted entirely to instructional materials, Estvan (44) pointed out that instructional resources are being defined more broadly and that they are more often seen as parts of a larger process of communication of ideas and attitudes to the learner. The amazing lack of research regarding text materials, apart from readability studies, was noted by Cronbach and his colleagues (34) who participated in a faculty seminar on the place and purpose of text materials in modern education. Several directions for possible research in this field are outlined in this report.

Current research on printed materials was reviewed by Otto & Flournoy (96). They also reported a preoccupation with the development or application of readability formulas. Staiger (116), for example, identified 10 factors related to readability of primary level readers, while Anderson (4) cited generally low correlations between readability scores and interest ratings for articles included in five general books of readings in psychology. A summary of the factors included and excluded in current readability formulas and a discussion of their use and interpretation with adults was prepared by Dale & Chall (37).

Audio-visual materials.—The contribution that audio-visual materials can make to effective learning has been supported by a considerable body of research evidence, but, as indicated in Allen's summaries of research in this field (2, 3), evidence on when such materials are most appropriately used is lacking.

The period covered by this review has been marked by the development of and experimentation with educational television techniques. However, only a few references can be cited. Carpenter & Greenhill (24) conducted a carefully controlled experimental study of the effectiveness of closed-circuit television for teaching three university courses. No significant differences were found in student achievement or attitudes toward the courses between students taught by the experimental procedure and those taught by the usual classroom methods.

The possibilities for adult education offered by educational television stations and special programs on commercial stations have been explored in several fields, with consistently encouraging results. Shimberg & Aird (109) found that home nursing could be taught about as effectively over TV as in the classroom. Evans and others (45) had similar results in teaching elementary biology and psychology.

Evaluation of instruction.—Bloom & De V. Heyns (18) have summarized a considerable body of recent research on educational achievement testing. The emphases that they noted, especially with respect to the development of new types of achievement tests and analyses of student responses to tests, were seen also in the period covered by this review. Attention has also been given to various factors affecting test performance, notably those related to school and community opportunities.

New evaluation instruments.—The lack of adequate evaluative instruments has been one deterrent to the study of problem-solving types of learning. A technique for studying the sequence of operations used in solving a problem, especially as they involve the use of sources of information, was developed by Rimoldi (103). Cross & Gaier (36) developed, using the tab test technique, the Balance Problems Test (BPT), a measure of relative preference for the use of facts versus the use of principles in solving problems.

The fine arts fields have posed special problems to the test constructor. Construction, validation, and standardization of a test of audio-visual discrimination, involving melody, harmony, and rhythm is, therefore, of particular interest. The development of this test, designed as a measure of musical achievement at college entrance for music majors, has been described by Aliferis & Stecklein (1).

The use of attitude instruments as measures of the outcomes of instruction is still far from frequent. Because of the importance of citizenship training as an instructional objective, the results of a study by Somit (115) should have a sobering effect on expectations of course outcomes. Differences in pretest and posttest scores of students in courses in political science and social science on three attitude instruments, involving interest in political

affairs, attitude toward politicians, and attitude toward personal participation in politics, were nonsignificant.

Factors affecting educational achievement.—The 1955 normative study of the Tests of General Educational Development, based on the scores of senior students in 1,506 high schools selected at random within state and school size strata from all senior high schools in the United States, provided an unusual opportunity to compare the general level of competence of high school seniors, as measured by these tests, in 1943 and 1955. Bloom (17) reports a significant increase in median scores on all five tests. Marked state-to-state differences in average performance were found to be related to financial support given to education, the use made of educational facilities, citizenship participation, and status of living in the state.

Mollenkopf & Melville (93) found that a best-weighted composite of community, school, and parent characteristics was not as useful as a predictor of achievement test scores for 9th and 12th grade students as a measure of academic aptitude. However, the percentage of a school's graduates going on to college, the size of the average instructional class, and the presence or absence of a community library did contribute to the effectiveness of prediction.

Methodology.—A well-developed discussion of the logic of construct validation by Cronbach & Meehl (35), while focused on personality measures, is also pertinent to certain types of achievement instruments.

Diederich (42) points to ceiling and regression effects as the most obvious reasons for the fact that college students with low initial scores gained most and those with high initial scores gained least in pretest and posttest analyses for a sizeable number of college groups on varied evaluative instruments. He also describes a third possibility, attributed to the construction of multiple-choice item responses, which makes it relatively easy to obtain sizeable score increments at low levels of performance, but difficult at higher levels.

A measure of partial information was obtained by Coombs, Milholland & Womer (31) with a modification of the usual multiple-choice procedure. Instead of asking the respondent to mark the correct response, the experimental method requires that he mark the distractors. Increase in reliability by this technique was particularly notable for the more difficult tests.

New measurement texts.—New texts for evaluation and measurement courses were published by Thorndike & Hagen (121), Travers (122), and Wrightstone, Justman & Robbins (131).

Educational guidance.—Important phases of educational guidance work, e.g., counseling techniques, prediction of student achievement, identification of exceptional students, are included in other sections of this review and in other chapters of this volume. Attention may be called here to the 1955 edition of *The Year Book of Education* (64), which includes papers on the historical and philosophical background of guidance, the areas of guidance, and the techniques and organization of guidance, predominantly by Ameri-

can and British writers but also by writers from countries throughout the world.

Of the many special problems of the guidance worker, one not otherwise covered, that received attention was the college student on probation for academic deficiency. Hackett (63) found no statistically significant differences in the achievement of counseled and noncounseled probationary students. He did find, however, evidence which would recommend concentrated counseling efforts for well-motivated probationary students.

Jones (72) found that the greatest differences between matched probationers and students of similar aptitude and home background lay in study habits. The practice of recommending reduced academic loads for students of low aptitude levels was not supported by the findings of a study by Andrew (6) in which a slight but positive correlation was found between academic load and academic success for students whose predicted grade point average on the basis of standardized tests fell within the probation category.

THE TEACHER

The alarming shortage of teachers has stimulated work on the personal and social variables that contribute to the choice of teaching as a career. While surveys of the "Why did you decide to become a teacher?" type have continued, some recent studies have focused on the family backgrounds which appear to encourage consideration of teaching as a vocation.

Anderson (5) found that parents of high school students of varying socioeconomic status differed somewhat in their ratings of advantages and disadvantages of the teaching profession but, on the whole, held generally similar attitudes. Ravitz (101) compared nursing and education students on several characteristics and identified some family and social background characteristics which were associated with the choice of teaching as an occupation. Kearney & Rocchio (76) found that the mean score on the Minnesota Teacher Attitude Inventory (MTAI) for senior high school students who selected teaching as their occupational choice was significantly higher than the mean score for those choosing nonteaching occupations.

Prediction of teaching success.—The prediction of teaching success has been one of the liveliest research areas in the educational psychology field. While far from answered, the many criticisms of previous research in this area, centering around problems of criteria and underlying theoretical conceptualization, appear to have had some effect. Research on the prediction of teaching success has been appraised critically by Lamke (79), Getzels (52), and Barr, Eustace & Noe (11). All three reviews urged continued research, though only the last detected some improvement in its quality with much more awareness of the importance of adequate criteria.

The critical review by Mitzel & Gross (92) of published studies on teacher effectiveness which use pupil growth as a criterion led to their identification of three types of problems to be considered and corollary suggestions for planning further research of this type: (a) the need for a multidimensional

approach to the definition of teaching effectiveness, (b) the associated needs for better definition of educational goals and development of adequate tools for measuring such outcomes, and (c) the need for controlling several factors in the design of experimental studies.

Stoelting (118) evaluated the various selective devices employed in selecting candidates for teacher-training in a university school of education against four criteria of teaching success and concluded that college grade point average was most promising. Knoell (77) replicated but contradicted an earlier study when she found that word fluency had little use in predicting teaching effectiveness.

Personality traits as measured by the Minnesota Multiphasic Personality Inventory (MMPI) were considered by several workers. Gowan & Gowan (56) developed a new scale for the MMPI designed to predict teaching success. LaBue (78) found significant differences on several MMPI scales between women students who completed teacher preparation and those who applied for admission but did not enroll. Cook & Medley (30) reported that teachers who score high, that is more favorably, on the MTAI tend to have higher elevations on the K scale than those who score low on the MTAI, whereas those who score low on the MTAI tend to score higher on the D scale and on obvious items.

Several studies have contributed to the validity data of the MTAI. Kearney & Rocchio (75) found a significant difference between mean MTAI scores for the 51 teachers liked best by students in four senior high schools and the 49 liked least. Della Piana & Gage (40) studied the relationship of pupil values on cognitive versus affective scales to their ratings of teachers and to the teachers' MTAI scores. The correlation between MTAI scores and pupil ratings was higher for pupils who held stronger affective values, while, for pupils with strong cognitive values, judgments of their teachers bore little relationship to the teachers' MTAI scores.

Interest patterns on the SVIB of student teachers, selected on the basis of supervisors' ratings as being in the top or bottom 15 per cent, were analyzed on a judgmental basis. Schultz & Ohlsen (106) reported that the best student teachers appear to be distinctive in their interest in working with people, their pursuit of intellectual interests, and in their preference for occupations involving teaching. The poor student teachers tended to prefer occupations involving personal gain rather than those involving teaching.

Using the Kuder Preference Record, Vocational Form C, Stewart & Roberts (117) found significant differences in mean scores on four scales (outdoor, persuasive, mechanical, and clerical) between women students admitted to a teachers college who dropped out during the first two years and those who continued through at least two years' work.

Ryans (105) reported that scores on the Teacher Characteristics Schedule, an empirically keyed self-report inventory, were correlated as follows with three patterns of teacher behavior in elementary classrooms: understanding, friendly (.49); responsible, businesslike (.41); and stimulating, imaginative

(.47). The corresponding correlations for a secondary teacher sample were .30, .35, and .16 respectively.

In comparing such studies as these with predictive studies in other professional fields, it is noticeable that much attention has been given to the prediction of success in actual teaching and relatively little to success in the teacher-training program. Although one may ask if the use of such ultimate criteria is justified at this time, one reason for such practice has been the uncertain relationship between achievement in the teacher-training program and subsequent success as a teacher. Also, promising attempts are being made to define the teacher's role and to develop more adequate instruments for evaluating effective teaching.

Roles of teachers and administrators.—Marked interest has been shown in the sociopsychological approach to research on teacher and school administrator behavior. Brookover (22) introduced an issue of the *Journal of Educational Sociology* devoted entirely to teacher and administrator roles with a schema showing the relationship between several aspects of role phenomena and an analysis of research dealing with teachers' and administrators' behavior. Bidwell (16) compared teachers' perceptions and expectations of their administrators with their degree of satisfaction with teaching. When teachers' role expectations of an administrator and their perception of his behavior converged, they tended to be satisfied with the teaching situation; the reverse was true when the role expectations and perceptions diverged.

Chase & Guba (28) prepared the first summary of research regarding administrative roles and behavior for the *Review of Educational Research*. Although they found increasing concern with the human relations aspect of the administrator's work, research studies were rare. They also noted that despite diversity in theoretical orientation much of the current work could be considered within the framework of role theory.

Educational administrators were compared with aircraft commanders with respect to two dimensions of leadership behavior in a study reported by Halpin (65). The hypothesis that educational administrators would rank higher on Consideration and lower on Initiating Structure was supported by the findings.

Several questions relating to the expectations generally held of the teacher role, the extent of conflict with other roles the teacher occupies, and the effect of such conflict on teachers who vary in certain personal and social characteristics, were explored by Getzels & Guba (53) in an interview and questionnaire study of 41 teachers from four school systems. Conflict ratings were found to be related to the sex, community relationships, and administrative relationships of the teacher and to his degree of satisfaction with teaching as a profession.

The development of role awareness, while accepted as an objective of field experiences prior to student teaching, has not been as well defined for preparing the teacher as it has been for the professional social worker accord-

ing to Miller (91). Self-evaluations and supervisor's evaluations of students participating in group work experiences aimed at such an objective have been used to indicate the potentialities of such an approach.

The effect of two types of teacher education programs on the student's concept of teaching was evaluated by Rabinowitz & Travers (100) by means of a drawing technique. Students at three stages in their work at two colleges, one offering a conservative academic program and one emphasizing professional training with carefully planned field and laboratory training, drew pictures of a classroom. The drawings were rated on five criteria by judges who were uninformed as to the college of origin. After training, significant differences were found in the extent to which the drawings portrayed pupil control, pupil-teacher tension, and pupil-teacher participation in some common activity.

Evaluation of teaching effectiveness.—A few recent studies have been more concerned with exploring the underlying variables of the teaching situation than with the evaluation of teaching effectiveness itself. Guba & Getzels (60) demonstrated the advantages of approaching the study of teaching effectiveness from a relevant psychological theory rather than the more traditional collection of information with little consideration of theoretical hypotheses. The results supported the following advantages for the theoretical approach: sharpened research objectives and criteria, simplified interpretation, minimized failure to detect important differences because of inadequate methodological technique, tested underlying theory, and facilitated comparison of separate studies.

A second example of a theoretically oriented study was reported by Gibb (54). He redefined Hemphill's nine dimensions of leadership behavior so that they were descriptive of classroom behaviors of college teachers and developed corresponding questionnaire scales. Four relatively independent factors were identified: friendly, democratic behavior; communication behavior; systematic, organization behavior; and academic emphasis.

Recognition that commonly accepted statements, such as that "Teachers should understand their pupils," need to be tested in terms of underlying assumptions, and that the variables implied by educational terms such as "teacher," "understand," and "pupil" need to be carefully defined is seen in a well-planned study by Gage, Leavitt & Stone (49). The general proposition that "Teachers should understand pupils," was tested with respect to three pupil areas: cognition, social, and personal problems. The social area was the only one in which the tested relationship was significant.

Elementary and high school level studies.—The problem of constructing a pupil reaction inventory for use at the elementary school level to measure something beyond a general attitude toward the teacher was explored by Medley & Klein (89) who identified three independent scales in such an instrument.

Observations of teacher behavior in the classroom have been related to personality measures by several investigators. McGee (87) found a correla-

tion of .58 between scores on a scale of authoritarianism (F-scale) and a specially devised measure of authoritarian trends in the teacher's observed behavior toward children in the classroom. Considerable originality was exercised by Willard (128) in developing instruments to measure teachers' values. The kinds of learning experiences provided by the teacher were indicated on a check-list by the principal, supervisor, investigator, and the teacher herself. The range of learning experiences provided by the teacher was found to be related to positive choices based on Wickert's list of normal goals of socialized adults.

Symonds (120) analyzed personal observations of the classroom behavior of teachers who had been identified as effective or ineffective on the basis of pupil ratings, verified by principal's judgment, and concluded that three personality factors differentiated the two groups. He later found that teachers in the same groups exhibited such different behaviors in the classroom that strictly observational techniques would be inadequate to distinguish the effective teachers.

College level.—The wide-scale development of instruments for student evaluation of college instruction has received considerable encouragement from the increasing interest in improving the quality of college instruction, and the consequent need for evaluative tools for the instructor's own use. An example of careful instrument construction, aimed primarily at such an evaluative purpose, is the forced-choice form described by Lovell & Haner (83). Ability and personality measures were compared by Bendig (13) for 16 instructors in introductory psychology in relation to the ratings by their students on competency and empathy.

GENERAL

Educational psychology texts.—Two new educational psychology texts by Garrison & Gray (51) and Lindgren (81) were published during the review period. Lindgren's is distinctive in its emphasis on group methods of learning.

Books of selected readings in educational psychology were compiled by Coladarci (29), Fullagar, Lewis & Cumbee (48), and Seidman (107). Indicative of the room for difference in viewpoint in the selection of materials for such books is the very small amount of duplication in the articles included in these three collections. A set of criteria for appraising books of readings in educational psychology was advanced by Gaier & Loree (50).

CONCLUSION

The tendency to neglect theoretical research and to concentrate on immediate and perhaps more practical problems, which may contribute little to the development of a systematic science, has been a frequent criticism of educational psychology. No marked change in this pattern of emphasis could be noted in the review period. Within a few areas, however, notably those concerning teacher and administrator roles, evaluation of instruction,

and classroom learning experimentation, some research workers have made serious attempts to relate research to underlying theory.

Concern about the responsibilities which educational psychologists have for theoretical development is not, of course, inconsistent with their responsibilities as applied psychologists. It is apparent that research efforts have been focused upon key problems of educators, notably better individualization of instruction to fit the particular student, more effective teaching of basic skills, more efficient use of teacher time, and better ways to evaluate learning outcomes.

The demand for an ever-increasing number of teachers has been reflected in studies of the factors related to choice of teaching as a career, the prediction of teaching success, and as a corollary of the latter, the development of procedures for the evaluation of teaching effectiveness. While such research has dealt with important problems, the findings have rarely been sufficiently conclusive to have wide application. Indeed, they are often most useful as sources of suggestions for further research. Experimental designs have often been inadequate, especially with regard to sampling, but also in lack of adequate controls and replication. On the positive side, though, several studies have explored common assumptions of educational practice and have raised pertinent questions about their validity. These reflect a healthy research attitude which should eventually contribute to the development of sturdier bases for educational practice.

Reasons for many of the deficiencies of educational research are found in the immediate needs of application and the inability to defer them while developing a proper theoretical framework. Two other factors might be considered: the limited financial support available for research in this area and the frequent demands made upon the time of many educational psychologists. The latter problem is not peculiar to the educational psychologists, though there is some evidence that they tend to take a more active part in the on-going committee and other activities of their own institutions as well as in the related work of state and local schools.

The announcement by the Office of Education (126) that it is interested in supporting research relating to the education of the handicapped, staffing of schools and colleges, the development of special talents of students, and the retention of students, all of which are important interests of educational psychology, presents one possibility of dealing with the financial problem. It is also likely, as Jones (73) has recently noted, that more extensive use might be made of financial sources that have up to now been used more extensively by researchers in related fields such as clinical psychology and mental health.

Certainly the problems faced by educational psychologists have shown no tendency to diminish. The publications of this period appear to demonstrate that these members of the psychological family are alert to these problems and active in their search for solutions. There is some evidence, too, that public schools are increasingly aware of their need for psychological

services. Such a climate should hold high promise for succeeding reviews in this field.

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PERSONALITY^{1,2}

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During the past year over 500 articles pertinent to the topic of personality have come to this reviewer's attention. While the volume alone would attest to the general health of research in this area, many important problems are but poorly represented. Research in personality as well as in psychology as a whole shows many of the characteristics of a fad. There will be a virtual flood of studies on a problem for a couple of years and then a new area or idea, catches the fancy of Ph.D. candidates. While the rash of experimentation serves in many cases to open up new areas of knowledge, all too often the work is mainly demonstrational in nature with excessive duplication of effort. And when the wave of enthusiasm moves on, there are frequently too few experimenters left to weed out and consolidate that which is theoretically useful and reproducible. The debris has still to be cleared from two of our recent fads—need-in-perception and the authoritarian personality.

With some notable exceptions, research tends to be characterized by one-shot experiments rather than programmatic attacks on a problem. While this leads to suggestive hypotheses, failure to follow up in many instances leaves uncertainty as to the reproducibility of the results and unclarity as to the relevant parameters. Experimental approaches to personality have become increasingly ingenious and sophisticated, but there also seems to be a decrease in scholarship. Too little effort is made by many researchers to relate their findings to those of other investigators. The necessary consolidation of results and reconciliation or pointing up of what seem to be contradictory findings are not currently being met by general review papers on topics of personality, and the breadth of scope required in an annual review chapter to a large extent precludes this function.

In the pages that follow, an attempt has been made to present a representative sample of current research although a preference for experimental articles and theories with a close kinship to empirical data has been exercised. The material has been organized under what seem to have become conventional subdivisions with the exception that theoretical and methodo-

¹ The survey of the literature for this review covered the period of May, 1955 through April, 1956.

² In this chapter the following abbreviations are used: A-scale (Taylor's scale of Manifest Anxiety); F-scale (California scale for measuring authoritarianism); GSR (galvanic skin response); MMPI (Minnesota Multiphasic Personality Inventory); Q-sort (method by which subjects sort self-descriptive statements into a predetermined number of piles according to a forced choice distribution); TAT (Thematic Apperception Test).

³ This chapter was written while the author was on the faculty of The Johns Hopkins University.

logical contributions have been treated under the topics to which they are germane rather than in a separate section. The amount of space devoted to particular topics and issues is not necessarily proportionate to their importance in personality research but reflects rather the relative amount of current research activity and the biases of the reviewer. Since these latter should be apparent to the reader, no attempt at a self-analysis will be given.

STRUCTURE

General traits.—A large part of research that is classified as "personality" is concerned with attempts to discover new dimensions along which to order people or to take components of old dimensions and re-combine them into new ones. In this area, Barron (6) has suggested some promising techniques for measuring a trait of originality. Defining originality in terms of an uncommon but adaptive response on a series of tests, he found significant intercorrelations among most of the originality measures, including observers' ratings. Originality scores in turn were found to be positively related to traits such as personality complexity, independence of judgment, and self-assertion. Drevdahl (53) has studied a similar dimension of personality under the label of creativity and has found differences between creative and non-creative graduate students on both mental ability and personality factors.

The relation of suggestibility to neurosis has been studied by Ingham (83). Although neurotics showed greater arm movement in response to suggestion, the difference was found to disappear when they were matched with controls for amount of arm movement without suggestion. Linton (104), on the basis of her research, concludes that several variables determine conforming behavior rather than a single trait of suggestibility, and Mouton, Blake & Olmstead (122) indicate that submissiveness may be one of these variables.

Kretschmer's concept of dissociation, or "Spaltungsfähigkeit," as a dimension underlying normal and abnormal behavior received some support from a study reported by Süllwold (157). However, in a much more extensive investigation, Payne (129) failed to find evidence for this trait. Despite the use of a large number of tests that supposedly measure dissociativeness, the number of significant intercorrelations was not greater than chance expectancy. Further evidence of perceptual differences between schizophrenics and normals has been reported by Lovinger (106). In line with previous studies, schizophrenics in poor contact were found to manifest less size constancy. Also, Johansson, Dureman & Sälde (85) have reported that schizoid or introverted subjects are less susceptible to a movement illusion.

A fruitful field for personality research is the large individual difference variance found in most psychological experiments. While experimentally-manipulated variables constitute a significant source of variance (at least in the studies that get published), percentagewise these variables generally contribute only a fraction of the variance as compared to individual differences. Block & Petersen (15) in a comprehensive study have investigated

some of the sources of individual differences in speed of decision on a judgmental task. While overly cautious people were found to be high on introspection and self-abasement, overly confident people tended to be rigid and dogmatic. Self-reliance and social perceptiveness were associated with realistic confidence in decisions, and passivity and conformance with speedy decisions.

Empathy.—The year's most extensively studied trait has been that of empathy or social perceptiveness. Research interest in this area has been intensive enough to insure empathy a place among the fads in psychology.

One of the disadvantages of a research fad is that often methodological weaknesses or problems that at first are not apparent vitiate a number of the first batch of studies. Campbell (25) has drawn attention to some of the statistical errors in empathy studies, and Hastorf *et al.* (79) conclude that "the use of predictions on attitudes or questionnaire items creates so many scoring problems that other experimental procedures should be investigated in a search for some feasible measure of empathy." The most comprehensive criticism of methodology in this research area, however, has come from Cronbach (41) and Gage & Cronbach (70). Cronbach has presented a thorough-going statistical analysis of the sources of variance in empathy scores and has shown the artifactual nature of many of the correlations reported between accuracy scores and other variables. From his analysis, Cronbach is able to point out some interesting and potentially useful aspects of social perception that had not before been readily apparent.

The article by Gage & Cronbach covers much the same ground except here the treatment is more general. The authors argue against the lumping together of predictions of diverse kinds of behavior either into a composite predictive accuracy score or into a general trait of empathic ability. They also stress the separately identifiable skills that are commonly required of judges.

The extent to which these criticisms apply to current research on empathy can be seen by examining some representative studies. Bieri, Blacharsky & Reid (13) report a tendency for adjusted subjects more accurately to predict similarities to themselves in others while maladjusted subjects were more successful in predicting differences. Interpretation of these results is confounded with the possibility that both adjusted and maladjusted subjects were making their predictions in terms of the group norms. Similarly, the finding that predictive accuracy is a positive function of similarity between predictor and predictee as reported by Wittich (172), Halpern (76), and Suchman (156) may mean nothing more than the subjects' tendency to make predictions in terms of their own responses.

Although their predictive accuracy scores are confounded with unidentified sources of variance, some studies in this area have suggested interesting avenues of investigation when more adequate scoring methods are available. Bieri (12) used Kelly's (92) role-construct theory to measure the number of different conceptual dimensions that subjects use in describing people they

know. Cognitive complexity was found to correlate positively with predictive accuracy and negatively with assimilative projection. A somewhat similar attempt to relate complexity of concepts with personality interpretations was made by Gollin & Rosenberg (72). Subjects who articulate and relate several molar features of personality-impression material were more likely to form hierarchic concepts when asked to say in what way abstract words like "communism" and "Buddhism" are alike. Stritch & Secord (155) have investigated the impression-formation process itself by determining the facial cues that are used in judging or forming impressions of personality, while Taft (159) has studied the social and personality characteristics of those who are good judges of others.

The rather important question of what value accuracy of social perception has for group behavior has been considered by Steiner (152). After reviewing some of the conflicting results that have been reported between empathy and group performance, Steiner points out some of the factors that determine whether empathy helps or hinders the group in the performance of its task. He concludes that accuracy of social perception is largely irrelevant where collective action occurs within the framework provided by role systems.

Rigidity.—While empathy may be said to be the personality psychologist's current love, rigidity is most certainly her rival. A sound survey of the recent literature on rigidity, problem solving, and stress has been presented by Oléron (126). As is apparent from his survey as well as from the studies themselves, different implicit and explicit criteria and definitions of rigidity are used by different investigators. Malmö & Wallerstein (111) postulate that rigidity can result from either reactive inhibition or inhibition attributable to competing responses. Some evidence was found that neurotic subjects were less susceptible to reactive inhibition, i.e., more rigid, than normals. A distinction between rigid and dogmatic thinking has been drawn by Rokeach, McGovney & Denny (134). In terms of Lewinian concepts, rigidity is referred to the barriers between psychological regions while dogmatism refers to the tightness of organization within a region. High- and low-rigid subjects were found to differ on the number of sets overcome while high- and low-dogmatic subjects differed on the number of sets that were integrated. Further research is indicated, however, to substantiate the validity of this distinction.

Wolpert (173), along with Frenkel-Brunswick (69), considers rigidity as possibly a defense or effect of anxiety and attributes his failure to find significant intercorrelations among various tests of rigidity as a result of the lack of anxiety in the testing situation. A commendable aspect of his research is his precaution in determining the reliabilities of his rigidity measures before intercorrelating them. French (67) has also failed to find significant intercorrelations among a number of commonly used rigidity measures. Her results offer no comfort for the hypothesis that rigidity appears only with anxiety since there was no difference in the intercorrelation of rigidity meas-

ures when the subjects performed under ego-involved as opposed to nonego-involved conditions.

A number of studies have continued to report correlations between specific rigidity measures and different aspects of performance. Overcoming sets on the water-jar problem has been related to certain aspects of goal-setting behavior in level-of-aspiration by Harway (78), and by Back (5) to performance in fact-gathering interviews. Back has suggested that performance on the water jars be viewed as a measure of approach to new problems in the face of learned solutions instead of structural rigidity, but this is suspiciously like recommending the substitution of a term for its definition. No relation of water-jar performance to impulsivity or overcautiousness in judgments was found by Levine (103), but Johnson & Stern (86) report a relation between subjective reports on experiences during intermittent photic stimulation and certain Rorschach indicators of rigidity. Buss (22) had previously analyzed certain components of rigidity into reversal shifts of positive and negative cues, and Takuma & Yoda (160) have confirmed and enlarged upon his finding.

Factor analysis.—The best methodology at our disposal for answering questions as to the meaning and generality of rigidity and other traits is factor analysis; yet relatively little use is made of this technique. We seem willing to investigate whether there are significant intercorrelations among a group of tests, but we do not often take the next step to get a more economical and informative analysis. More unfortunate is the general neglect of the findings from factor analytic studies in the design of experiments. Too much of the research on personality correlates of learning, decision making, political attitudes, etc., is conducted with personality inventories and tests that have uncertain validities and at least several separately identifiable sources of variance. The failure to use unidimensional criteria in such studies makes it almost impossible for proper theoretical consolidation to take place. If scores on an ideology scale, let us say, are actually composites of three different but unidentified factors or sources of variance, we cannot logically conclude that performance A and performance B are related just because the scale has been found to correlate with both performances. Not only does the failure to use unidimensional tests prevent proper generalizations and consolidations from taking place, but it also tends in many instances to mask and obscure true correlations between personality traits and objective aspects of performance.

Apparently undaunted by the general neglect of factor analytic findings, Schaie (137) has undertaken a factor analysis of a battery of different tests of rigidity and perseveration currently in use. In a well-designed study he finds that behavioral rigidity seems best described by three factors, interpreted as motor-cognitive rigidity, motor-cognitive speed, and personality-perceptual rigidity. There are some encouraging similarities between these factors and those previously reported by Cattell & Winder (31).

In the area of general traits, Cattell (28) has summarized the literature

on factors found by *P* technique (correlation within persons) and has related it to findings from his own extensive investigations. Seven factors recognizably recur in independent investigations, and there is suggestive evidence for five other factors. For the most part these factors are matchable with the major factors found using *R* technique (correlation between persons). In another report, Cattell (27) covers the evidence for the factors underlying objective personality tests. Here there is evidence for 18 factors, 12 of which have an appreciable amount of variance associated with them. Again there is some evidence of these factors matching with factors found using other types of test material. Cattell & Gruen (30) also find evidence of the same factorial personality structure in children. While many of the factors are faint and their measurement uncertain [Stephenson (153), for example, has argued that factors from rating-scale data are apt to exist more in the conceptions of the raters than in the behavior rated], they represent at this stage the best contribution that factor analysis has for personality research. Despite their many inadequacies, they are superior to the heterogeneous collections of items called scales that are too frequently employed in current research. The actual value of these personality factors, as well as their limitations and hope for improvement, lies in more general research usage.

Eysenck (60) has proposed a theory of cortical inhibition to account for the observed behavioral differences underlying introversion-extroversion, one of three dimensions in his factorial description of personality. He has hypothesized that dysthymics and hysterics who represent the introversion and the extroversion poles differ on the rate of build-up of reactive inhibition in the nervous system. [Note that Malmo & Wallerstein (111) have also used the concept of reactive inhibition in accounting for rigidity.] According to his theory, inhibition builds up fast in the hysteric or extroverted personality and relatively slowly in the dysthymic. He deduces and experimentally confirms that on figural aftereffects hysterics will develop a stronger satiation and more quickly than will dysthymics. He also presents a number of other interesting deductions from this theory as to differences in performance between the hysterics and dysthymics. One of particular interest is that dysthymics will condition more rapidly than hysterics. Anxiety neurotics are at the dysthymic pole in Eysenck's system, and there has been ample evidence that subjects with high scores on anxiety scales condition more rapidly than subjects with low scores. This relation between Eysenck's theorizing and the current anxiety scale research is of further interest since it has previously been pointed out that the anxiety scale is related to an hysteria-psychasthenia dimension [Deese, Lazarus & Keenan (49); Eriksen & Davids (58)], and this relation seems to be confirmed by Franks (65).

In other factor studies, Lorr & Rubinstein (105) determined a set of factors that were descriptive of psychiatric outpatients. Factors descriptive of temperament, dynamic traits, and symptoms were found. For dimensions of emotion, Ekman (54), using a rather novel technique of having subjects

rate similarity between pairs of emotional words, found 11 factors, 9 of which were interpretable.

Syndromes.—Personality types are still with us. Evidence continues to be advanced that there are enough instances where several traits are sufficiently clustered in a large enough number of people to justify and make useful carefully qualified personality types such as the authoritarian and various Freudian character structures. These typologies continue to be controversial as to validity and value. Farber (63) reports a correlation of .37 between a five-item scale on anal-retentive attitudes and political-aggressive attitudes. Stagner & Moffitt (151), on the other hand, cast doubt on the appropriateness of using Freudian typological formulations. They show that the correlation of traits within groups of individuals typifying the Freudian character types is not significantly greater than the between-type correlation.

Evidence for a syndrome of "alienation" has been advanced by Davids (42, 43). Assessment by clinical interviews and projective tests indicated that the traits or dispositions of egocentricity, distrust, pessimism, anxiety, and resentment were interrelated. People who were high on any one of these traits tended to be high on all of them. The syndrome was also revealed in the selective perception and recall of statements with alienation type content.

The authoritarian syndrome continues to represent the most vigorous attempt to relate attitudes to general personality characteristics. A new scale more personality- and less ideologically-oriented has been offered by Webster, Sanford & Freedman (167) for the study of authoritarianism. Although the new scale is reported to correlate .74 with the F-scale, there is some question as to whether the relationships found with the F-scale will hold good for this new scale. The reported correlation leaves nearly 50 per cent of the variance unaccounted for, and there is considerable indication that the F-scale has three or more separate sources of variance.

Meer (119) has studied the relation of the F-scale to aggression in dreams with out-group and in-group characters. Although subjects with high scores show more aggression with out-group characters in their dreams, this result must be accepted cautiously. Of the numerous ways of comparing the aggressiveness of dreams, Meer selected only ratings on strength of the aggression which may have capitalized to an unknown degree upon chance. A relation between authoritarianism and leader acceptance among members of Air Force Radar Squadrons was reported by Medalia (118), and French & Ernest (68) found that F-scale scores were related to acceptance of military ideology. French & Ernest have also indicated the nonunidimensionality of the F-scale since the relation of F-scale and acceptance of military ideology was greater when the items dealing with projectivity, sexual concerns, and superstitions were excluded from the F-scale scores. The relation of empathy or social perception to authoritarianism has been investigated by Scodel & Freedman (141), and Dowling (52) has confirmed previous indications that

both high and low scores on intolerance of ambiguity are associated with maladjustment and neurotic symptoms. In addition, Masling, Greer & Gilmore (114) have related authoritarianism to sociometric choices.

The relation of authoritarian ideology to parent-child relationships and discipline has also been examined. Kates & Diab (91) related scores on the F-scale and several other measures of ethnocentrism and tolerance of ambiguity to attitudes on parent-child relations. Some significant relations were found for female subjects but not for the males. Punitive attitudes in children were found to correlate with the children's form of the F-scale in a study by Lyle & Levitt (108). This relation existed even when a negative correlation between the F-scale and IQ was partialled. It is to be noted that in both studies the correlations, while tending to be significant, were of a low order of magnitude.

There is an ever-increasing amount of evidence that indicates the authoritarian syndrome has not only been overgeneralized, but also that questions some of the underlying assumptions. Confusions over the meaning of authoritarianism and its confusion with authority were pointed out by Gregory (74). Also the content of the F-scale items has been extensively used to indicate and define attitudes of authoritarians and nonauthoritarians; yet two independent studies have shown that a large part of the variance of F-scale scores is due to a response-set bias. Bass (9) and Leavitt, Hax & Roche (101) point out that the F-scale is entirely made up of positively-worded items such that the authoritarian answer represents agreement. Leavitt *et al.* found that when half of the items were reworded in the negative direction, the split-half reliability of the scale, instead of being high negative, actually yielded positive correlations for some samples. Bass factor analyzed his data and found that 75 percent of the reliable variance of the F-scale was associated with the acquiescence factor and only 25 per cent with the content factor of authoritarianism.

The concept of a tolerance-intolerance of ambiguity dimension that exists in perception as well as in other personality and social relationships has played a crucial role in tying together the diverse aspects of the authoritarian personality. Recent research, however, has questioned whether such a dimension runs through the various psychological functions. McCandless & Holloway (109) found no demonstrable relationship between racial prejudice in children and six operational definitions of intolerance of ambiguity, and Davids (44) failed to find significant relationships between F-scale scores and various measures of ambiguity tolerance for visual and auditory perceptual material.

There have also been a number of directly conflicting results reported from attempts to relate F-scale scores to other measures. Davids (44) reports a correlation of .69 between the F-scale and the Taylor A-scale while French (67) fails to find a significant relation. Conflicting results such as these suggest that too little attention has been paid the limiting conditions under which the authoritarian syndrome holds. The meaning of authoritarian

ideology with respect to the rest of the personality might well vary with different sub-cultures. Davids' results were obtained with Harvard undergraduates where the general culture places emphasis upon liberal tolerant attitudes while French's data were obtained from Air Force enlisted men. If one assumes that anxiety symptoms and neurosis are associated with failure to assimilate the cultural standards, this disparity in findings might be reconciled. Too little attention also has been paid the effect of having a Jewish versus a non-Jewish experimenter upon results obtained with high authoritarian or ethnocentric subjects. The relation between the experimenter and subject is a major part of the conditions of any study on humans, and this variable has been previously shown to be important in opinion and attitude polling.

A major value of the authoritarian personality studies has been the attempt to understand attitudes in terms of personality theory. This approach has been followed and expanded in a volume by Smith, Bruner & White (145) where 10 normal men were intensively studied in order to determine "what function is served in each life by holding a particular set of opinions about Russia and Communism." While the study was essentially exploratory, the data are a rich source of insights and hypotheses. The work as a whole represents a significant step forward in the understanding of opinions and their formation.

Self-concept.—An application of learning theory to the self-concept has been made by Helper (81). In boys the relations of the ideal self to the parents' ideal for the child as well as self-concept modeling were positively related to an index of parental reward for similarity to the father. Similar relations for similarity to the mother were not significant for girls. Manis (112) has assumed that the self-concept is not essentially different from other attitudes or collections of beliefs, but his experimental results give only partial confirmation to this hypothesis. Some interrelations between the self and role-taking were reported by Sarbin & Jones (135), and Lundy *et al.* (107) report that people who are liked correspond to one's ideal-self description whereas people who are disliked correspond to the unliked characteristics of the self. Similarly, McKenna, Hofstaetter & O'Connor (110) report that the subject's ideal self resembles the perceived friend more closely than it does the perceived self.

In addition to the perceived and the ideal self, Freedman (66) has measured the projected self as revealed in TAT protocols. For normals, the intercorrelations among the three selfs is moderately positive, but neurotics show lower correlations of the ideal with the perceived and projected self. Paranoids were distinguished by low correlations of projected with ideal and perceived self. Block & Thomas (16) have shown that degree of self-satisfaction is a curvilinear function of adjustment, but ego control shows a monotonic relation with the satisfaction variable. Since paranoids may be presumed high on ego control even though poor on adjustment, these findings would appear to mesh well with Freedman's. The context of self-ratings was

shown by Cowen (38) to determine negative self-concept scores, and Jourard & Secord (88) have studied the role of body-cathexis in self-concept and personality.

While the *Q*-sort continues to be the common measure of the perceived self, Sarbin & Rosenberg (136) have found an adjective check-list useful in obtaining a qualitative estimate of the self. Interesting differences were found not only between normals and neurotics but also between males and females and members of different religious groups. Osgood's semantic differentials were made use of by Solley & Stagner (147) in determining high and low self-evaluations. They found that behavior in response to barriers was in part determined by the value of the perceived self. Somewhat similar conclusions were reached by Lepine & Chodorkoff (102) who found that goal-setting behavior was a function of expressed feelings of adequacy and correspondence between perceived and ideal self.

Changes in self-description have been frequently used in studying the course of psychotherapy. However, a research by Taylor (161) suggests considerable caution in attributing such changes to the therapeutic process. The intensive self-introspection involved in performing repeated *Q*-sorts without any intervening psychotherapy resulted in the same kind of changes in the self-description that have been found to accompany therapy. While the changes he observed were not as great as those with therapy, the lesser magnitude of the changes might be attributable to differences between "normals" and those ready to enter therapy. For example, people entering therapy might be expected to have less positive self-evaluations so that on future self-assessments there is greater opportunity for them to express more positive attitudes toward the self. Taylor's results indicate the need for an adequate control in future evaluations of therapy via this or some similar technique.

ENVIRONMENTAL AND HEREDITARY VARIABLES

The effect of infantile experience upon later personality has been the most interesting question in this research area during the year. The only conclusion available at this time is that the research results are controversial both for animal and human studies. Weininger (168) reports that early handling of rats results in increases in weight and ambulatory behavior and less emotional behavior in new situations; however, Scott (142), in a well-controlled study, fails to confirm any of these findings. Similar confusion reigns with respect to the importance of mothering for human infants. The studies of Spitz (149) have in general been accepted by psychologists as demonstrating the importance of mothering during infancy in producing normal emotional health in infants and adequate emotional responsiveness in later childhood. That this acceptance has been too uncritical and perhaps unjustified has been shown by Pinneau (130). In a critical examination of the Spitz studies Pinneau raises questions and points out defects in design, methodology, and statistics that would seem to leave Spitz's conclusions, at

best, only suggestive. In the inevitable series of replies to replies (131, 148), Spitz, in this reviewer's opinion, fails adequately to meet the questions that have been raised. While most of us will continue to believe in the importance of mothering during infancy, we must recognize that this belief has more the characteristics of a faith and less the basis of demonstrated fact.

Blau & Blau (14) studied behavior following long and short bottle feeding sessions in an infant during the ages three to seven weeks. Contrary to theory it was found that the long-feeding sessions resulted in more nonnutritive sucking and were followed by more crying, general restlessness, and sleep difficulty than the rapid-feeding sessions. However, it is possible that the long sessions might have resulted in more gastric distress as a result of more swallowed air getting farther into the gut before terminal burping.

The relativity of symptoms and behavior standards to culture has been brought to our attention again, this time by Slotkin (143). As the clinician should be well aware by now, standards for normality cannot ignore a cultural and subcultural referent. Differences in attitudes to dirt and getting dirty between middle- and lower-class children were studied by Alper, Blane & Abrams (2). In a very clever experiment, they compared the finger-paint and crayon productions of nursery-school children from the two subcultures. As expected, the middle-class children showed a lower tolerance for getting dirty, staying dirty, and for the products they produced while dirty.

The relation of personality to body-build still remains an unsettled issue. Using Sheldon's somatotypes, Davidson *et al.* (48) found some characteristics of body-build that differentiated between Oxford students with mental illness and two control samples. There seem to have been enough general findings of this nature to justify more extensive work on determining the value of Sheldon's system. Cattell, Blewett & Beloff (29) have made use of a new multiple-variance analysis method to study the relative contribution of nature and nurture to 12 personality factors. While laboring under many handicaps, both statistically and sample-wise, this remains an ambitious undertaking and for many of the factors studied presents the best evidence currently available.

MOTIVATION

Motivation, probably the most central and pervasive problem in psychology, continues to be elusive and difficult to conceptualize. A marked contribution has been made by the Nebraska Symposium Series on Motivation through provision of opportunity and impetus to individual researchers to organize and make public their thinking. The current edition of the *Nebraska Symposium on Motivation* (115), the third in the series, contains papers by Maslow, McClelland, Olds, Peak, Rotter and Young. Compared to the previous two symposia the recent one is characterized by broader and more general approaches.

Several of the participants, notably McClelland and Olds, lodge some effective protests against the traditional tendency to lump together, under

the single concept of motivation, many separately identifiable aspects of behavior. McClelland argues against the view that all motives are functionally equivalent and draws attention not only to species differences but also to the differential predictive power that different types of human motives have for the psychologist. From a more physiological point of view, Olds makes distinctions between needs, drives, rewards, and punishments. His review of an exciting series of physiological studies lends substantial support to his distinctions.

One of the more important points made by Olds is the separation of need and drive as separate concepts. Others, of course, have made a similar kind of distinction, but such a conceptual distinction seems to be increasingly popular. Hebb (80) has separated the drive or energizing function of motivation from the need or stimulus aspects which in his system serve mainly to guide or direct. However, in line with recent research in both physiology and psychology, Hebb has modified his position. While the separation is maintained, he now proposes that sensory events not only serve the cue function but also have an arousal or vigilance role. The report by Thomae (164) of periodic cycles in children's play interests and in their rest and activity periods suggests the nature of some of the interactions between stimuli and drive.

Taking a lead from the animal studies suggesting an exploratory motive, Cohen, Stotland & Wolfe (36) have investigated a need for cognition in humans. Their face-valid measures of this need were found to relate to the degree of frustration experienced with ambiguity, and a lack of a relation between need-cognition and need-achievement was reported. Buss, Durkee & Baer (23) have examined the meaning of hostility. Different aspects of hostility such as resentment, assault, and suspicion were found to have low interrelationships, but all were related to over-all ratings of hostility. Davids *et al.* (47) have studied the assessment of aggression in clinical interviews, projective tests, and self-reports and found rather complex interactions between the different assessment procedures and scores on the direction and amount of aggression.

Schwartz (140) has inquired into the validity of the psychoanalytic motives of castration anxiety and loss of love by examining the effect of film material dealing with these respective areas upon subsequent TAT stories. No significant effects were found on the scoring categories assumed to measure loss of love, but six of the scoring categories for castration anxiety showed a difference in the expected direction. Unfortunately, the scoring categories most obviously related to castration anxiety (loss or damage to genitals, bodily mutilation, and sexual inadequacy) did not differentiate. In a subsequent study (139) some of these castration anxiety measures were found to differentiate the TAT's of a normal from a homosexual sample.

Anxiety and stress.—The problem of anxiety and its effects has received its share of experimental attention. The most extensive work is that of Basowitz *et al.* (8) who studied the stress of paratrooper training with both

physiological and psychological techniques. At both levels of investigation measures were found that were sensitive to what turned out to be a relatively mild stress situation, and the results add appreciably to our understanding of stress on both the biochemical and psychological levels.

The effect of anxiety upon discrimination was studied by Eriksen & Wechsler (59). Using a design which permitted the separation of the response from the perceptual factors in discrimination, they found that anxiety resulted in stereotypy and increased predictability of response, but there was no evidence that anxiety affected the perceptual process, *per se*. Smock (146) found that the experimental introduction of anxiety results in higher recognition thresholds for pictures with incongruities, and Beam (10) made use of real-life stress situations such as impending doctoral examinations and found some very marked effects upon serial learning and GSR conditioning. Under stress, errors were increased as much as sixfold in serial learning, and conditioning was more rapid. One wonders, however, whether similar effects might not have been obtained with subjects anticipating a very pleasant experience and required to perform an irrelevant nongoal related task. In further work, Hörmann (82) reported the interesting finding that certain of Klein's (94) perceptual styles were related to individual differences in performance under mild stress.

There has been little progress in sorting out the different factors in stress and its effects upon performance. While the different kinds of stress and methods of induction may all be assumed to produce anxiety, it is likely that the nature of the performance effects would differ depending, for example, upon whether the subject was threatened with electric shocks while actually performing the task or whether the stress was produced by circumstances entirely unrelated to the experimental situation. One might also hesitate in generalizing the personality traits found by Korobow (96) to relate to speech under delayed auditory feedback to other stress performances. While subjects conceivably become anxious on this task, the observed correlations with personality traits may be related to individual differences in speech skills and dependence upon auditory and kinesthetic feedback.

Anxiety as currently conceived has several aspects and research is indicated to determine which of the aspects of anxiety is important in producing different performance effects. Kamin *et al.* (90) have attacked this problem experimentally in an attempt to find the dimensions involved in the anxiety concept. They report evidence for an "avoidance" and an "arousal" factor. The role of muscle tension should also be considered since it is clinically one of the defining properties of anxiety. That muscle tension may be responsible for many of the reported effects of anxiety upon performance has been previously noted by Meyer (120) and recently by Matarazzo, Ulett & Saslow (117) and also by Kuethe (97). Kuethe studied the stereotypy and predictability of responses under neutral conditions and conditions where subjects performed under the threat of electric shocks and experimentally controlled muscle tension. Both muscle tension and threat of shock were found to affect

responses in the same way and both conditions had a similar interaction with scores on the psychasthenia scale from the MMPI. Bourne (19) has reported that induced tension facilitates responses, and it is to be noted that there are marked similarities between results obtained in the recent studies on anxiety and the older studies on muscle tension so well summarized by Courts (37).

There is evidence to indicate that the findings from anxiety studies may not be specific to anxiety but may be characteristic of drive states in general. This is implied in the Hullian concept of drive, and the muscle tension research indicates another line of evidence. Bartoshuk's (7) results support his position that increases in motivation are associated with an increase in muscle tension, and Olds (115) has also indicated that drive as he conceives it may be reflected in muscular tonus. In addition Hebb (80) has related his conception of drive or arousal to an inverted *U*-function between strength of arousal and efficiency of performance that is quite similar to the inverted *U*-function found between muscle tension and performance. Whether or not future research proves that muscle tension is a common denominator accounting for many if not most of the performance effects under high motivation, we still have too little evidence to conclude that given performance effects are specific to particular motives.

Experiments comparing high- and low-scoring groups from the Taylor A-scale are still popular. Kerrick (93) investigated the relation between A-scale scores, IQ, and discrimination in high school students. Subjects who were high on both A-scale and IQ tended to be less discriminating in that they used the end and middle categories of a 7-point rating scale most often while subjects low on both IQ and A-scale used the intermediate scale positions more frequently. A marked difference between high and low A-scale groups was reported by Stevenson & Iscoe (154) on the number of trials to solve a simple size-discrimination problem with the high group requiring more trials.

Speech behavior in response to TAT cards was compared between high and low A-scale subjects by Benton, Hartman & Sarason (11). No differences were found in rate of words nor in number of adjectives or verb to adjective ratio, but the high group tended to use more words and to have a shorter latency in response to the TAT card. Davids & Eriksen (46), however, found that high subjects gave significantly more chained associations than low scoring subjects in a twenty-second interval which indicates a rate difference, at least for this type of task.

Matarazzo *et al.* (117) report a rectilinear relation between A-scale and number of trials to criterion on a human maze. High scores required more trials, but the relation between time to criterion and A-scale was an inverted *U*-function with both high and low subjects requiring more time than subjects with median scores. Taylor & Chapman (163) report that subjects high on the A-scale learn paired associates more rapidly than low subjects when the list has a minimum of intraserial duplication. Lazarus, Deese & Hamilton (98) who had earlier reported this same finding will be pleased to see their

results confirmed. Some questions, however, have been raised concerning the relation of A-scale scores to maze learning performance. Axelrod, Cowen & Heilizer (4) failed to confirm the findings of Farber & Spence (62). To add to the confusion over personality correlates of learning performance they found that the Lie scale from the MMPI related to maze learning performance.

The validity of the A-scale as a measure of anxiety has been further studied by comparing the scores of normal and psychiatric groups. Buss (21) performed an item analysis of the scale with clinical ratings of anxiety as a criterion, and Matarazzo, Guze & Matarazzo (116) compared the scores of medical and psychiatric patients. These latter authors found that the scale differentiates with reasonable accuracy between the two patient populations but fails to differentiate within the psychiatric group. They also point out that the scale has not been shown to be a valid measure of anxiety, *per se*. The validity of the Sarason Test Anxiety scale was investigated by Martin & McGowan (113) with the finding that high scoring subjects show greater GSR's during a discussion of their attitudes toward examinations than do low subjects. Since the scale is supposed to measure anxiety and tension over examinations, this finding reflects favorably upon the test's validity.

Interpretation of performance differences between high and low A-scale subjects continues to be ambiguous. Farber (61) defends the assumption that the scale measures drive level in terms of the consistency of the empirical results with other theoretical assumptions. While this is certainly suggestive, scientific method requires that the adherents of this view adequately demonstrate that the obtained interrelations are not the result of other plausible factors. Differences in performance between highs and lows cannot with confidence be attributed to the drive properties of anxiety since on the face of it the scale measures the frequency with which subjects have experienced anxiety or anxiety symptoms in the past. This provides no guarantee that the subjects are anxious at the time of the experiment.

More serious, as far as interpretation is concerned, is the multidimensionality of the A-scale. O'Connor, Lorr & Stafford (125), for example, have found five separate factors in the A-scale. Also the scale has been shown in the past to correlate with a number of different personality inventories and traits, and one may reasonably argue that performance differences associated with A-scale scores are attributable to one or more of these other traits or factors.

Kamin (89) reports a significant negative correlation between the A-scale and a test of mechanical information. He raises the question whether the results obtained with the A-scale may not be attributable to apparatus stress since subjects less familiar with mechanical gadgets are more likely to become anxious in the presence of mysterious experimental equipment.

The relation of A-scale scores to intelligence continues to be a source of controversy. Calvin *et al.* (24) report a correlation of $-.31$ between A-scale score and Wechsler IQ, but Taylor (162), Davids & Eriksen (46), and Klugh

& Bendig (95), fail to find significant correlations using such measures of intelligence as the ACE (American Council on Education) and grade point averages. The findings of Schulz & Calvin (138), however, are somewhat ambiguous. While they report a correlation of .00 between A-scale and ACE, they also state that they confirmed the linear relation between A-scale scores and intelligence. If so, this is one of the neatest tricks of the year. The contradictory findings with respect to the relation of the A-scale and intelligence are perhaps a result of differences in undergraduate populations and to different attitudes in taking the scale. But, even if the scale is found to correlate with intelligence, the magnitude of the correlation cannot be very large in college populations, and we are still faced with the question as to whether the assumed anxiety affects intelligence test performance or vice versa.

Lack of unidimensionality is not a criticism unique to the A-scale but applies to most of the scales and inventories used in personality research. As an example of the kind of interpretive difficulty that results from this practice, we have an experiment by Williams (170) in which speed in simple addition was studied as a function of success and failure, self- as opposed to experimenter-determined goals and scores on the Interfering-Tendency Questionnaire, the A-scale, and Achievement Imagery in the TAT. Complex interactions were found between the inventory or scale scores and the experimental variables, but a clear interpretation of the results is confounded with the pattern of intercorrelations among the inventories. A significant correlation existed between Achievement Imagery and Interfering Tendencies, and there was a correlation of .64 between the latter and the A-scale.

Johnston (87) has reported an interaction between achievement imagery scores and neutral and threat-of-shock conditions on learning scores that appears to be quite similar to the type of interactions reported by Deese, Lazarus & Keenan (49) and Kuethe (97) between A-scale scores and performance. Also these same or very similar measures have been found to relate to completed and incompleting task recall (3, 55). In fact we are confronted with an extensive body of data showing the relation of objective performance measures with personality variables but are unable to draw any firm conclusions because of the lack of knowledge concerning what, if anything, these different performances have in common. Significant advancement in this research area would no longer seem to permit the luxury of the haphazard use of personality inventories. Perhaps factor analysis would lead to some meaningful consolidations and should be undertaken.

DEFENSES AND CONFLICT

Defense mechanisms.—Most of the work on repression consists of attempts experimentally to demonstrate its existence rather than to analyze and relate the phenomena to other psychological knowledge. The main characteristics of the current year's research crop is the increase in the general level of

sophistication in the selection of material that would be apt to activate repressive processes. Cowen, Heilizer & Axelrod (39) selected descriptive adjectives on which individual subjects had indicated a discrepancy between the way they were and the way they ideally would like to be. The results show that more trials were required to learn these "conflict adjectives" than to learn adjectives on which the subject had indicated close agreement between the real and ideal self. Similar results were obtained by Cartwright (26) who found that self-consistent material was better recalled than inconsistent, and also that the difference in recall was related to maladjustment.

That the defensive processes, whatever their eventual explanation, do not depend upon differences in initial learning, is indicated by Flavell's study (64). Decrements in recall occurred when threat or anxiety was not associated with the material until after initial learning. Removal of the anxiety association, however, did not restore recall performance. Jacobs (84) in his experiment took account of the individual differences in defensive behavior. He found that while learning emotional words was more difficult for the average subject, the amount of blocking on the learning tasks was also related to score on the GAMIN Inventory. These recent studies taken together with the large number that have preceded them are quite convincing that repressive-like behavior can be produced in the laboratory. If we can now accept this point as demonstrated, future research can busy itself with analysis of the phenomena. There is a good chance that a thorough experimental analysis of the defensive behavior produced in the laboratory will go farther in explaining repression than further attempts to produce a demonstration that meets all of the somewhat nebulous criteria of Freudian repression.

One technique that offers promise for the experimental study of defense mechanisms is the perceptual recognition experiment. The weight of evidence shows that personality defenses as appraised by independent techniques also can be observed in recognition thresholds. While this much seems proven, there is still controversy over explanations for these effects. An explanation solely in terms of differences in frequency of past occurrence can be discarded. Not only have the same effects been obtained in studies that had adequate controls for frequency, but recent research has indicated that the role of frequency in recognition and other similar behaviors has been grossly overestimated. Allport (1) has rejected frequency as an explanatory concept after an extensive analysis, and Wiener (169) has shown that the recognition for words like "balls," "pussy," and "screw" will vary with whether they were recently experienced in a neutral or sexual context. Davids (45) failed to find that frequency was an important variable in determining number of chained associations, and Jacobs (84) failed to find correlation between Thorndike-Lorge frequencies and either reaction times or GSR's to words. Bresson (20), however, in a review on frequency and motivation, concludes that such concepts as perceptual defense and subception are

essentially *ad hoc*, and their usefulness in clinical psychology does not justify bringing them into the perceptual area. Such a conclusion is perhaps understandable in view of the rather limited evidence surveyed by Bresson.

Nelson (124) has presented some interesting evidence for perceptual defense. Four Blacky pictures were simultaneously exposed in a tachistoscope, and subjects were asked to identify the pictures in two of the positions designated by the experimenter. Subjects were independently rated for the presence of conflict and the use of avoidance or repressive defenses, and it was found that those subjects with repressive defenses called or named the conflict picture significantly less often. Blum (18), using a technique very similar to Nelson's, concludes that "current attempts to abandon perceptual defense in the interests of 'theoretical parsimony' may very well be premature." He interprets the experimental data in this area in terms of unconscious perception by which ego defenses are alerted.

It is to be noted, however, that an acceptance of the proposition that defense mechanisms are revealed in perceptual recognition experiments does not necessarily require an assumption of unconscious perception. The problem of unconscious perception has been largely responsible for the controversy in this area, and the problem itself has arisen primarily because of a failure to distinguish perception both operationally and conceptually from the responses by which it is inferred. The term "perceptual defense" is unfortunate and misleading since there is no experiment known to this reviewer that contains the necessary converging operations that would permit a conclusion of defense in perception, *per se*. Postman (132) has long argued for an interpretation in terms of response variables, and recently Garner, Hake & Eriksen (71) have argued not only the need for a conceptual distinction between perception and response but have also pointed out some of the converging operations by which such distinctions can be made. In the experiment by Nelson (124) described above, one could predict his results by assuming lower response strengths for the names of the conflict pictures (attributable perhaps to these responses being conditioned stimuli for anxiety arousal) without invoking any characteristics of the perceptual process. A necessary but lacking control in his experiment would be to show that the same results could not be obtained when subjects are tachistoscopically shown only pictures other than the ones portraying their conflicts. If the same results were obtained in this control condition, an interpretation in terms of unconscious discrimination would be meaningless since the conflict pictures were not present to be unconsciously perceived.

Well-known facts on avoidance conditioning and the effects of punishment show that responses to which anxiety has been conditioned have a decreased probability of occurrence. Eriksen & Browne (57) have made use of these principles in providing a behavior theory account of perceptual defense which eliminates the need for an assumption of unconscious perception. They have assumed along with Dollard & Miller (50) that the subject's

own verbal responses, both implicit and explicit, can be conditioned stimuli for anxiety arousal. Thus, such responses would have a decreased probability of occurring. The results obtained by Eriksen & Browne support their analysis. A similar explanation of perceptual defense is implied by Solley & Stagner (147), and Vernon (165) has discussed the perceptual defense data in relation to schemata in perception.

While the data of perceptual defense studies may not require the use of unconscious discrimination for their explanation, still the problem of unconscious processes and their contribution to behavior remains a legitimate and important question. Unfortunately, sound experimental approaches are lacking for such investigations mainly because of poor definitions of awareness. Eriksen (56) has argued that the subception experiment of Lazarus & McCleary (99) is inconclusive on the question of discrimination without conscious awareness. The experiment did not provide a control to show that the verbal responses permitted the subjects were adequate in number for the subjects to reveal all the discrimination they were capable of making at the verbal level. If we only allowed subjects to respond "black" and "white," for example, we would never have any evidence that they could verbally distinguish shades of gray. Eriksen goes on to analyze the subception experiment in terms of a partial correlation model, a formulation which focuses attention upon sources of noncorrelated errors in different but concurrent response systems.

While subception may be artifactual, there are other recent experiments indicating that perception and behavior are determined by stimuli and cues which are not available to the subject's verbal report. Smith & Henriksson (144) in a well-designed and controlled study have shown that perceptual illusions can be produced as a result of prior stimulation that is apparently not represented in the subject's awareness. Also the current studies on the control of verbal behavior through social reinforcement have confirmed Greenspoon's (73) report that verbal responses can be manipulated without the subject being able to state how his behavior has changed or what the reinforcement was. Taffel (158) was able significantly to influence the choice of pronouns in sentence construction by saying "good" although the subjects lacked insight as to the reason for their pronoun choices, and Razran (133) reports that salivary conditioning was greater among subjects who were unaware that they were being conditioned. That verbal manipulation is not limited to isolated word responses is indicated by Verplanck (166) who found that statements of opinion could be manipulated through subtle social reinforcement. Not only do these studies indicate the need for psychology to face up to the problem of awareness, but they also raise the question as to whether the changes in patient's verbalizations during psychotherapy may not be a result of subtle reinforcement from the therapist.

Although most of the experimental work has been done on repressive types of defenses, there have been several interesting and encouraging at-

tempts to relate mechanisms such as displacement and identification to learning theory principles and general psychology. Murray & Berkun (123) have related approach-avoidance conflict to Miller's theory of displacement and stimulus generalization. The distance rats would travel in an alley toward a punished food cup was found to be inversely related to the spatial similarity of that alley to the alley in which they had been punished. It was further shown by excerpts from therapy sessions that the manner in which a patient brought up topics of anxiety could be rather neatly described in terms of displacement and approach-avoidance conflict.

Lazowick (100) has made use of Osgood's conception of meanings in arriving at a definition of identification. While imitation is defined as a similarity of behavior between subject and model, identification is a similarity of mediating processes between the two.

The L- and K-scales from the MMPI were used by Page & Markowitz (128) to measure "defensiveness." Asked to rate an interviewer who gave them "failure" information in regard to performance on a pseudo-intelligence test, subjects with high defensive scores were much harsher in their ratings than low-defensive subjects. Similar evidence of the projection of blame was found by Doris & Sarason (51). A high- and low-scoring group, selected on the basis of scores on the Test Anxiety Scale, were given a failure experience on a pseudo-intelligence test and asked to check the reasons for their failure on a rating scale that was provided. Some evidence was found indicating that high scores were associated with a greater readiness for self-blame. Solley & Stagner (147) have found a similar kind of behavior associated with positive and negative self-evaluation, and some previous results with anxiety-scale scores and performance under stress have indicated that low-anxiety subjects are more defensive and less willing to admit personal inadequacy.

A novel and intriguing approach to the study of defenses has been made by Cohen (35). He studied the social interaction of people as a function of similarity and dissimilarity of their conflict areas and their defense mechanisms. Using the Blacky test to assess conflict areas and defense mechanism, he found that projectors interacting together rated their interaction more negatively than pairs who utilized other defenses. Also there was a general tendency for subjects using avoidance defenses in common to experience their interaction as more positive. A different approach to the study of defenses has also been made by Wispé & Lloyd (171) who studied the desire for structure in relationships with superiors among sales personnel of a life insurance company. They suggest that the desire for structured personal interaction is a defense mechanism which attempts to control the behavior of those in authority positions.

Additional contributions on defensive processes have been made by Clark & Sensibar (33) who have reanalyzed some previously published data on the expression of sexual needs in the TAT. Whereas in the previous report the TAT stories were analyzed on the basis of manifest sexual con-

tent, the present study is concerned with symbolic sex references. Symbolism in dreams was investigated by Hall (75) who was specifically concerned with the meaning of the dream of being attacked and of falling. From a content analysis of 106 dreams, Hall concludes that dreams of being attacked are not the expression of castration fears but are rather the expression of the fears of an impotent person unable to cope with threats.

Frustration.—Children's responses to repeated mild frustrations as a function of need for power-dominance and love-affection were observed by Otis & McCandless (127). Aggressive behavior was found to increase with number of frustrations while the amount of submissive behavior decreased. A significant negative correlation was found between ratings on the need for power-dominance and love-affection. An increase in aggressive behavior with frustration was also reported by Block & Martin (17) in a well-designed replication of the Barker, Dembo, and Lewin experiment. They also found that ratings on "ego control" predicted the behavior of the children to frustration. Cervin & Ketchum (32) found an initial increase in performance level following frustration with a later decrease when frustration became greater. TAT stories were found to reflect the effects of frustration in an experiment by Crandall (40). Following a frustration the TAT protocols showed a decrease of striving in the area where the frustration occurred, with some evidence that the decrease generalized to related areas.

The frustration-aggression hypothesis has been re-examined by Haner & Brown (77). They distinguish between drive strength and habit strength in the formulation of the hypothesis and in an experiment show the effects of drive level upon the instigation to aggression. Cohen (34) had subjects describe what they thought was the actual and ideal behavior in response to descriptions of frustrations. Less aggression was found in the ideal answers and in response to the less arbitrary frustrations, but the generality of these findings is limited to what people think they would do since actual behavior was not investigated. Stagner & Congdon (150) report a failure to demonstrate displacement of aggression in a study similar to the classic one in this area by Miller & Bugelski (121). Arbitrary failure on an intelligence test failed to produce any significant shifts in attitudes toward minority groups.

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SOCIAL PSYCHOLOGY AND GROUP PROCESSES¹

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How good was the past year's work in social psychology? Only the foolhardy would venture a reply, for the answer depends upon the criteria of judgment employed, and consensus about criteria is not remotely possible in this field. Recognizing that any evaluation must be personal and controversial, this reviewer has nevertheless tried to screw up enough courage to make a few evaluative judgments about the work of his colleagues.

To start where it is safest, most psychologists will agree that research activity is a good thing, that the field will not advance unless a lot of people are in there trying. On this score the past year was outstanding. Although an exact comparison of the quantity of publication with previous years cannot be made because there is no unequivocal definition of the field, at least as many relevant publications have been uncovered this year as before. Compared to 10 or 20 years ago, social psychology is today a veritable beehive.²

What is all the activity about? Compared to 20 years ago it is devoted heavily to solving theoretical problems. In manner of reporting, at least, it is hypothetico-deductive. And this trend is especially interesting because so much of the research has been motivated, or at least financed, out of a need to improve social practice. Apparently a reconciliation of practical and theoretical interests is being achieved through a more basic attack on the practical problems. Some of the most theoretically oriented research of the past year was financed by business or military organizations.

This is not to say that the theorist's millennium has arrived. On the contrary, one is impressed by the dearth of integrative theorizing, and the field seems to be crying for construction of theoretical systems which will encompass the vast supply of accumulated findings. While this task is not an easy one, it may not be insurmountable in the near future because current work is clustered around certain theoretical constructs. What is needed is a grand synthesis in which the various constructs and limited models are made integral parts of a more comprehensive conceptual system.

Just how far social psychology has come along the road to theoretical maturity may be illustrated in the way attitude tests are employed today. A not completely unfair characterization of the early work in a major segment of social psychology would be that it consisted simply of devising and

¹ This review covers the period between May 1, 1955 and May 1, 1956.

² A regrettable consequence of the quantity and diversity of this output is that it cannot be fully comprehended by a single person. This review, it should be noted, is deficient in its treatment of non-English language publications, a defect of increasingly serious proportions as social psychology expands around the world.

administering attitude tests. Relatively little was done with these data conceptually. Recently, the descendants of this tradition have become busy administering one attitude test again and again to the same subjects. Current interest centers not so much on the responses as on the relationships among them. These relationships, either among one subject's responses to different administrations of the same test or among different subjects' responses to the test, are taken as indicators of higher order constructs. Thus, "acceptance of self" is indicated by the discrepancy between responses under instructions to report one's actual characteristics and to report one's ideal characteristics, "empathic ability" is measured by the congruence of one's predictions of how others will respond and how others actually do respond, and various aspects of "resistance to change" are revealed by comparing one's responses under repeated administrations over time. A cynic may see in this development slight cause for comfort, but it would seem that this shift to higher order constructs is a sign of growing maturity. In any event, the change has resulted in considerable confusion, and much of this year's output either reveals such confusion or is concerned with reducing it.

As attention shifts to higher order constructs it appears to be moving away from pure description. There is relatively less effort to discover how attitudes are distributed in the population and correspondingly more interest in the functional relations among higher order constructs. Does "empathic ability" predispose one to be chosen as leader? Does salience of membership heighten resistance to change of group-anchored attitudes? Such questions are asked, and answers given, as if it made no real difference what population is being studied.

This development raises again an issue which has never been adequately settled: what is the proper approach to sampling when the objective is to determine functional relationships rather than the incidence of some attribute in a population? If old fashioned social psychology was satisfied with college sophomores, modern psychology is little different. Today, other captive subjects have been added from the military, business, and high schools, but very few findings are based upon random samples of specific universes. Current practice is either to ignore the problem and state findings in completely general terms or to make a gesture to the problem by stating that the findings cannot be generalized. Neither approach is satisfactory. Even those fortunate researchers with a random sample of some national population should not rest at ease, for social psychology cannot be content with a collection of national generalizations. Must we wait for a random sample of all the people who will ever live on the earth before we can safely generalize to the nature of man? Intuition suggests that confirmation of findings from widely different populations and from experimental manipulation of variables does lend confidence to generalization. Chapman, in his review of the Norwegian work of Rommetveit (110), hints that this is so when he says that "it comes as another demonstration out of Oslo that either an internationally unified social psychology is emerging or at very least that a sort of *pax americana* is at

work" (18). Clearly, we need a defensible rationale to govern the way in which research findings are to be generalized.

Probably the greatest source of tension for social psychologists at the present time is the question of how to relate the traditional disciplines of psychology and sociology. Both disciplines have converged upon a common set of problems, and while it is easy enough to guess the disciplinary training of a particular researcher from his vocabulary and the "great names" he cites, it is much more difficult to be sure that he is really saying anything different from what he would if he had been trained in the other discipline. The resulting conflict over definitions causes enough trouble, but the real problem is how to handle sociological and psychological variables in a single conceptual scheme. The extremists would treat social systems as patterns of learned behavior or individual behavior as manifestations of the operating characteristics of social systems, but the extremists are in the minority. Most writers who venture into the fray seem to hope to develop a system of thought which encompasses both kinds of variables.

No clear solution has yet emerged. But judging from this year's work, sociological variables usually enter in one of four ways. Properties of social systems may be thought of as (a) stimuli to which individuals respond perceptually, (b) patterns of behavior to be learned or "internalized," (c) ecological variables which facilitate or hinder an individual in his goal striving, or (d) a space in which the person's location determines how others react to him. Each of these approaches has its own special difficulties. Theoretical progress in social psychology will depend to a considerable extent upon clarifying further the proper way of conceptualizing relationships between these two kinds of variables.

SOCIAL PERCEPTION

"Social perception" first appeared in the index of the *Psychological Abstracts* in 1945, but already there is an extensive literature. The tremendous activity generated by the "early" papers of Dymond (30) on "empathic ability" and Chowdhry & Newcomb (19) on "social sensitivity" makes one think of a driver racing wildly over unpaved roads with little concern over the condition of his steering wheel and tires. The posting this year of a few "danger" signs, therefore, occasions some sense of relief. Symptomatic of the new caution is the conclusion reached by Hastorf, Bender & Weintraub (54) that even the "refined empathy score" of Hastorf & Bender (53) is not entirely satisfactory and that "it would, perhaps, be opportune at this time to express our growing conviction that the use of predictions on attitude or questionnaire items creates so many scoring problems that other experimental procedures should be investigated in the search for some feasible measure of empathic ability."

A frontal attack on current methods of measuring sensitivity and empathic ability was launched in two papers by Gage & Cronbach (43) and Cronbach (26). They point out that the typical experimental design contains

four components: (a) the Judge whom the experimenter is trying to measure, (b) the Other(s) whom the Judge is asked to interpret, (c) the Input or information available to the Judge, and (d) the Outtake or statements about the Other obtained from the Judge. With the aid of this scheme it becomes clear that studies which apparently have been measuring the same thing, e.g., "social sensitivity," are actually not comparable. Consider Input and Outtake. Different investigations have differed considerably in the amount and kind of Input given the Judge and in the degree of inference required from Input to Outtake. Such studies could hardly produce comparable results. Perhaps most useful methodologically is the critique of those commonly used scoring procedures which are based on (a) the Judge's self-description, (b) the Other's self-description, and (c) the Judge's prediction of the Other's self-description. Employing these indicators, three concepts can be defined: Real Similarity as agreement of (a) and (b); Assumed Similarity as agreement of (a) and (c); and Accuracy as agreement of (b) and (c). Since only two of these are independent, reported findings may be nothing more than logically necessary correlations. Finally, it is shown that a composite accuracy score is a poor measure of sensitivity because it may be influenced not only by relations between Real and Assumed Similarity but also by the following: (a) Elevation, or the difference between Judge's central tendency and the central tendency of self-descriptions for all items and Others combined; (b) Differential Elevation, or Judge's ability to estimate deviations of the Other's elevation from the average; (c) Stereotype Accuracy, or ability to predict the profile of item means both as to shape and scatter; and (d) Differential Accuracy, or ability to predict differences between Others on any item.

In the light of this analysis, one can hardly be surprised by the finding of Suchman (134) that estimation accuracy is a function of the characteristics of the estimator, estimatee, group, situation, and feelings being estimated. It is difficult to interpret the report of Jackson & Carr (60) that a control group reveals greater empathic ability and assumed similarity to others than a group of schizophrenics. And it is just as hard to know what to make of the material presented by Bell & Stolper (7) showing that the empathy test of Kerr & Speroff (70) is not related to Dymond-like tests of empathy nor to one that they designed. The agonizing reappraisal, started by Cronbach and Gage, should be continued.

Social perception and leadership.—As might be expected, efforts to relate social sensitivity to leadership still produce conflicting results. According to Smith, Jaffe & Livingston (128), the greater the agreement in a group between an individual's ratings of the power of each member and the average rating received by each member, the higher will he be rated on effectiveness by an objective observer, the more will he be valued by the others in the group, and the higher will be his mean rating on power. Trapp (142) finds that two sociometrically determined leaders of a sorority are more successful than others in predicting responses of members to the Picture-Frustration Test.

Browne & Shore (13) report that supervisors are superior to nonsupervisors in predicting the attitudes of different classes of employees, that those in "more focal" supervisory echelons predict more accurately than those in less focal ones, and that there is no significant tendency to predict more accurately attitudes in adjoining or near echelons. In contrast, Gage, Leavitt & Stone (44) can find little support for the proposition that grade-school teachers should understand their pupils, since nearly all correlations between measures of teachers' understanding and pupils' ratings of their teachers are insignificant.

Steiner (131) attempts to account for the generally conflicting results which relate accuracy of social perception to group efficiency by proposing that these should be related only when (a) the group members are motivated to co-operate, (b) the accurately perceived qualities are relevant to the activities of the group, (c) members are free to alter their own behaviors in response to their perceptions of others, and (d) the behavioral changes which result from accurate perception produce a more integrated dyadic system. If a group has a clearly specified role system, these conditions are not well met, and accuracy of interpersonal understanding should then have no relationship to group efficiency. While previous studies have not reported data explicitly related to role systems, it would seem that the findings might conform to this conclusion. However, all studies which have measured sensitivity as the discrepancy between number of sociometric choices received and guesses of the number of choices received are suspect, as Campbell (15) points out, because the insight scores are made up of actual choices received plus guessed choices, producing a spurious correlation.

Determinants of social perception.—On the whole, studies of determinants have displayed a greater ingenuity of method than have those attempting to measure accuracy of perception. Gollin & Rosenberg (48) present further results from a motion-picture technique previously described by Gollin (47). Subjects were asked to recount the events of a movie in which a young woman is shown in a series of discrete and diverse behavioral activities which intermingle two themes, promiscuity and kindness. They were also given a concept-formation task developed by Rokeach (109). It was found that those who articulate and relate the two themes are more likely to form hierarchic concepts than are those who articulate their impressions without relating the various aspects, or those who fail to deal with both traits. It appears that impression formation and concept formation are guided by similar organizing tendencies.

A rather different organizing principle is examined by Lundy (87) who adopts a view advanced by Kelly (69) that in a situation where two people are talking to one another a dimension of "incorporation-differentiation" becomes important. Thus, a person reacts to another either by incorporating the other into constructs he holds about himself or by differentiating the other person from these constructs. Experimental results show that post-interaction characterizations of the partner become more like self (projection)

when the predictor focusses attention on self during interaction, and more like the partner (greater accuracy) when the predictor focusses on him.

The finding of Fiedler, Warrington & Blaisdell (37), that subjects perceive those they like best as more similar to actual and ideal self than those they like least, is questioned by Lundy *et al.* (88) who show that acceptance of self modifies the results. Subjects who attribute unacceptable descriptions to themselves tend to see these traits also in those they like least.

"Object" and "subject" determination are distinguished by Bjerstedt (9) in his analysis of "pal descriptions" made by Swedish children. Object determination is measured by intersubjective consistencies and subject determination by intersubjective differences. The objective analysis reveals that highly chosen children are seen as socially positive, socially active, and effective in work, while the subjective analysis exposes a tendency to perceive members of one's own sex more positively than those of the opposite sex.

Five widely different investigations illuminate other determinants of perception. Secord & Jourard (123) had judges rate their mothers and four groups of photographs of women's faces on 20 traits. They show that a judge's mean rating of the photographs differs from the mean rating of other judges in the same direction that his mother-concept deviates from the modal mother-concept and that the attribution of mother traits occurs more frequently for photographs which in general are rated as having motherly traits. According to Clarke & Campbell (20), white pupils significantly underestimate test scores of Negro classmates, Negro pupils do not err systematically, and a monetary reward for accuracy significantly increases accuracy. Using a Q-sort device, Block & Bennett (10) measured the degree to which a person's appraisal of his various two-person social situations coincides with the evaluation of the same situation by the other participating individuals. "Consensual accuracy" turns out high for interactions with friends but low for interactions with statused professionals. They call attention to two kinds of interactional distortion, one of decoding where the individual does not receive messages being sent, the other of encoding where the individual does not send the interpersonal messages he intends. Scott (122), defining accuracy of perception of organizational relationships as the agreement of reported relations with those specified by an official organization chart, finds that accuracy declines as a member of a submarine squadron describes relations with his immediate superior, his subordinates, and his peers and that the perceptual field tends to be "vertical" rather than "horizontal." A common type of error is the tendency to perceive oneself too high in the organization. Finally, Thibaut & Riecken (136) studied a situation in which the subject is confronted with a person having relatively high or relatively low power who complies to an influence attempt made by the subject. They find that the perceived causal locus for compliance is "internal" to high-power and "external" to low-power persons. Furthermore, after compliance there is a greater increase in acceptance of the high- than the low-power person.

ATTITUDES AND OPINIONS

Attitudinal research in the past year proceeded along well established paths with interest centering on test construction, scaling, statistical relations among items, and techniques of changing attitudes.

Methods.—Although the method of equal-appearing intervals, originally developed by Thurstone & Chave (139), is by now a standard technique, there remain questions about its adequacy. Webb (144) has reviewed the literature and come to certain conclusions. (a) The assumption of equal-appearing intervals is not justified; the scale values should be regarded as order statistics. (b) Reliable scale values can be obtained with fewer subjects and intervals than Thurstone first used. (c) The ambiguity index (Q values) probably contributes little in addition to the scale values. (d) The findings regarding the assumption of independence of scale value and judges' attitudes are conflicting. One of the studies most seriously questioning this assumption was that of Hovland & Sherif (59). Further work, reported by Kelley *et al.* (66), shows that the influence of judges' attitudes is virtually eliminated when comparative rather than absolute judgments are required. Thus, the method of paired comparisons or that of successive intervals scaling achieves independence of judges' attitudes. Relative judgments, it seems, reflect a semantic continuum while absolute judgments reflect a continuum of social significance or importance. That extremely different groups can conceive the structure of attitudes similarly is shown by Messick (98) who applies a new technique to Air Force officer candidates and students at a theological seminary. Although these two groups hold widely different attitudes toward war, capital punishment, and the treatment of criminals, they reveal a definite similarity in the ways in which they believe attitude items are related within people.

Toman (140) describes a new "multiple attitude test" in which the subject sorts 60 statements concerning 12 categories of attitudes on three bases of sorting. The results are interpreted according to the patterns of choices for each category on the three sortings. Characteristics presumed to be revealed are such things as ambivalence, reaction formation, and inhibition. Krugman (72) attempts to disentangle two processes involved in resisting propaganda, one the evasion of propaganda messages and the other resistance to change after receiving a message. The method consists of obtaining measures of anxiety, tension, and readiness to be exposed to messages counter to own attitudes. Neither of these new techniques has yet an adequately demonstrated validity.

Nature of attitudes.—Two approaches to characterizing the nature of attitudes are evident in the literature. One hopes by statistical procedures to discover the dimensions or structure of attitudes. The other relies more directly on psychological theory and tries to define the properties of constructs.

Within the factor analytic approach two studies have appeared which question the unidimensionality and universality of radicalism-conservatism.

Williams & Wright (149) present data in support of McNemar's contention (95) that this "dimension" is actually composed of several independent components. Their data, based on a cross section of the adult population of Elmira, New York, yield two independent factors, called Threat Orientation and Group Identification. Either factor might seem to reflect liberalism-conservatism. The generality of Eysenck's finding (33) that radicalism-conservatism is a cross-cultural factor is questioned by Keehn (64) on the basis of data collected from Lebanese students. Although he does find Eysenck's factor of Tough-Tender Mindedness, his second factor could better be interpreted as Arab Nationalism than radicalism-conservatism. Keehn concludes that radicalism-conservatism is probably best viewed as a special case of a more general tendency to structure attitudes according to the major political issue of the culture. If this interpretation is correct, it would appear from the data of Williams & Wright that the major political issues in American culture center around party or class identification and ethnocentrism.

The construct-definition approach is well represented by Peak (103) who defines attitude as a process which involves affect organized around a conceptual or perceptual nucleus. The affect of any given attitude, according to this view, is determined by the instrumental value of the reference of the attitude and, through a process of generalization, by the affect attached to similar referents. Three consequences stem from this instrumental conception of attitudes. (a) The affect of an attitude is some function of the judged probability that its referent leads to good or bad consequences and the intensity of the affect expected from these consequences. Peak shows that a person's attitudinal position can be predicted from a number of indices based on a combination of judgments of the satisfaction to be derived from reaching various goals and the instrumental relation of the attitude issue to these goals. (b) If an object or situation is seen as positively instrumental to a valued end, then the greater the satisfaction of attaining that end the more favorable will be the attitude toward the instrumental object. If it is seen as interfering, then the greater the value of the goal the more negative the attitude will be. But if it has no relation to the goal, altering the value of the goal will not change the attitude. Preliminary findings support these predictions. (c) It follows that the affect attached to an object can be changed by changing either the perception of the probability that the object leads to goals with various satisfaction potentials or the satisfaction attached to any goal that is already seen as related to the object.

The third prediction is tested by Carlson (16) who had college students provide three sets of data before and after a change procedure: (a) measures of certain attitudes, (b) ratings of 25 general values in terms of expected satisfaction from each, and (c) ratings of the 25 values in terms of the probability that the referent of one of the attitudes (allowing Negroes to move into white neighborhoods) would lead to, or block, the attainment of each value. The change procedure successfully modified perceptions of the role of Negro housing segregation in attaining four values. In keeping with the predictions,

changes in attitudes were related significantly to changes in the perceived instrumental relationships and to changes in an index based upon both satisfaction and instrumentality. It should be noted, however, that subjects with extreme attitudes did not show sufficient change to permit definite conclusions about the applicability of the theory to extreme attitudes. Carlson's attempt to verify the hypothesis of generalization of affect by similarity yielded ambiguous results. Eisman (32), however, reports a successful demonstration of mediated generalization in which she obtained an increase in preference for stimulus B by teaching children to attach the same label to stimuli A and B and then rewarding responses to A.

Another conception of attitude, expressed in terms quite different from Peak's but saying much the same thing, is outlined by Cooper (24). He conceives of attitudes as determined by the individual's perception of objects as barriers or facilitations and predicts relationships between the social class assigned to various categories of people (doctors, Negroes, etc.), the valence of these categories, and the social approval or disapproval the person would expect to receive from associating with members of the category.

Change of attitudes.—Several papers emphasize the importance of keeping separate "comprehension" and "acceptance" of the persuasive message. Thistlethwaite, de Haan & Kamenetzky (137) point to ambiguity in the findings of Hovland & Mandell (58) that conclusion-drawing by the communicator facilitates attitude change. In repeating and extending the earlier study, it was found that conclusion-drawing produces greater comprehension of the intended conclusion but no greater change in attitude than does no-conclusion-drawing. The discrepancy between the two sets of findings can be explained if it is assumed that Hovland & Mandell's measure of attitude change was measuring comprehension to some degree. Thistlethwaite & Kamenetzky (138) pursue this same distinction in extending the work of Hovland, Lumsdaine & Sheffield (57) on the effectiveness of "one-sided" versus "both-sided" arguments. Here they fail to get consistent results when comparing "one-sided" versus "both-sided" arguments, but they find that attitude change tends to be greater for those who more clearly comprehend the communicator's intended conclusion or who show fewer or less intense discounting reactions to the communication. They conclude that introducing facts in support of the "other side" will reduce effectiveness when these facts are not already familiar to the audience but that failure to introduce well-known facts on the "other side" will weaken the appeal. "Comprehension" may also be important in interpreting the finding of Janis (61) that subjects who score high on "test anxiety" are more susceptible to persuasion, since it is possible that these subjects pay more attention to the persuasive material and are thus more likely to remember it. That retention of an attitudinal statement is better when the subjects approve of it and believe it to be true is reported by Garber (45) although his results do not attain high levels of statistical significance.

Two studies reported by Weiss & Fine (147, 148) test the hypothesis that

more aggressive people will be more influenced by a punitively oriented communication. In the first study level of aggressiveness and tendency toward extrapunitiveness were related to changes in attitudes toward the use of punitive measures in dealing with juvenile delinquents. Reading an article favoring punitiveness brought about greater attitude change toward punitiveness among subjects high on aggressiveness and extrapunitiveness. No significant differences were found, however, between level of aggressiveness and factual comprehension of the article. In the second study aggressiveness was experimentally aroused in subjects before they read the article. Aroused subjects were more likely to change toward greater punitiveness after reading the article but only on issues directly dealt with by the article. This result is not consonant with the finding of Miller & Bugelski (99) that, after frustration but with no specific incitements, subjects assign more unfavorable traits to certain nationality groups. All of these findings are hard to relate to the neat demonstration by Farber (34) that aggressive political attitudes are significantly associated with anality.

The problem of source credibility was reopened by Cooper (23) who asked students to write an anonymous letter describing as objectively as possible the alleged misuse of funds by vice-presidential candidate Nixon. These letters were then shown to another group of students who attempted to guess the political affiliation of the author of each letter and to evaluate the letter as objective or slanted. Ability to identify the true political affiliation of the authors significantly exceeded chance, and subjects tended to evaluate as objective those letters which they thought were written by authors of the same political affiliation as their own. Pastore & Horowitz (102) add support to the generalization that statements attributed to more highly approved authors are more readily accepted. They show, too, that more praiseworthy motives are attributed to more highly approved authors and that statements are more readily accepted when seen as generated by praiseworthy motives.

Social anchorage of attitudes.—Studies of resistance to change of attitudes are coming increasingly to examine the way in which a person's relations to other people serve to stabilize his attitudes and opinions. Kelley (65) advances the hypothesis that when "group-anchored" attitudes are exposed to counterpressures their resistance to change will be greater when the group has higher salience for the individual. His results support the hypothesis for Catholic high school students but not for Catholic students in a metropolitan university. It is not clear that the necessary conditions were created among the university students to provide an adequate test of the hypothesis. Another attack on the same general problem is reported by Kelley & Woodruff (67) who reason that if resistance to change does stem from the anchorage of opinions in a group it should be possible to reduce resistance by conveying information that other members of the group have changed their opinions. In an ingeniously designed experiment, members of a college group listened to a recorded speech contrary to their norms and heard an audience applaud the main points of the speech. This audience was described for some subjects as

composed of members of the college group and for others as composed of anonymous outsiders. Those who believed that the applause came from their own group were found to be more likely to change their opinions but also to misinterpret the speaker's point of view as being more similar to their own.

A rather novel approach to the problem is advanced by Brodbeck (12) who analyzes the way in which a person uses a group to maintain confidence in his opinions. More specifically, she presents evidence for the view that (a) individuals whose confidence in a belief has been shaken by exposure to propaganda prefer to hear arguments from their own side in order to bolster their confidence and (b) that given an opportunity to participate in a free discussion after the exposure, these individuals tend to listen preferentially to persons who agree with them. Although the experiment does not provide evidence for the point, she also proposes that if the communication causes conversion, then the person will tend to seek discussion with others who support the propaganda.

The part played by face-to-face interaction in determining the impact of mass communications is analyzed by Katz & Lazarsfeld (63) in their study of Decatur, Illinois. Opinion leaders were located with respect to four topics: marketing, fashions, public affairs, and movie-going. For each of these it was found possible to show that opinion leaders tend to differentiate themselves from nonleaders in social status, gregariousness, and place in life cycle, but the relative importance of each of these traits is different for each topic of influence. Furthermore, relatively few people are leaders for all topics. The relative impact of radio and television versus primary groups was assessed by Forer (41) on the basis of questionnaires given to 2700 high school students. He found extremely favorable reactions to a program which presented a panel of teen-agers giving advice on teen-age problems. The common view that teen-agers are more influenced by peer groups than by other socializing influences is shaken by responses to the question as to whether they would follow advice given on the program before that given by other people. Only 10 to 15 per cent say they would follow the advice of the program before that of mother, siblings, father, and minister, rabbi, or priest, but 49 per cent would follow the program's advice before that of their teen-age friends.

A dramatic demonstration of the social support of attitudes is presented by Fleishman, Harris & Burt (40) in their study of the immediate and long-range effects of an intensive foreman training school. One purpose of the school was to instill attitudes and behavior displaying "consideration," and the immediate effects of the school were successful. However, after the foremen had been back on their jobs a few months, they showed less consideration than before they attended the school. The interpretation advanced is that management was actually rewarding a lower level of consideration and that the persisting effect of training was to make the foremen more responsive to management pressures. This interpretation is supported by the finding that foremen whose immediate superior is more considerate are themselves more considerate. Similar processes seem to account for the findings obtained

by Fishman (39) in his study of stereotypes of majority Americans held by Jewish children who have received different kinds of Jewish education. Except for one group, the stereotypes are quite homogeneous across groups, and this exception disappears among older children. Fishman attributes these results in part to the homogeneous pressure from the larger culture which tends to reduce the effects of the different educational programs.

Several studies of international relations and cross-cultural education have focussed attention on the conflicting pressures that derive from multiple group membership. A collection of studies edited by Smith (130) elaborate upon the processes involved, and Watson & Lippitt (143) employ the concept of multiple group-membership to account for the tensions and resistance to change displayed by a group of foreign students in America. The essentially negative findings of Smith (129) concerning the change of attitudes of American students visiting Europe may well reflect the same processes. Guetzkow (51) has developed an extensive theoretical analysis of the problem of multiple loyalties as they influence the development of international relations.

SOCIAL INFLUENCE PROCESSES

A thorough understanding of the way in which attitudes are anchored in the social field or of the way in which behavior is organized and directed by groups and institutions requires knowledge about the processes of social influence. During the past year a great deal of research was undertaken to advance such knowledge. Probably the most significant publication was the theoretical analysis of social norms and roles by Rommetveit (110). While this book presents many empirically testable hypotheses, the most timely aspect of the analysis is its treatment of "sent norms" and "received norms." This distinction makes it possible to remove much of the ambiguity in treatments of norms and roles as "shared expectations" and to deal with these within a theory of social influence.

The extensive literature on convergence of judgments in social situations which has accumulated since the publications of Sherif (125) and Asch (3) is reviewed by Mausner (91). He lists six types of determinants: nature of stimulus being judged, instructions given to subjects, history of contact between subject and others present, reaction to others in the experimental situation, personality of subject, and subject's previous experiences with the stimuli or class of judgments. Two theories are outlined. One is perceptual or cognitive and stresses such variables as stimulus ambiguity, judgment scales, and perceived prestige of those in the experiment. The other concentrates on response tendencies and views the judgment situation as a conflict between tendencies to continue within a previous judgment range and to agree with the others present. Prior reinforcement is the major concept of this theory. While the distinction between perceptual and response theories is useful, it is not always easy to employ. Two papers by Luchins & Luchins (85, 86) appear to fit the perceptual theory except that they emphasize the importance of "foretraining." Specifically, they show greater conformity to another sub-

ject's incorrect judgment after a foreperiod in which the experimenter consistently indicates that the other subject's judgments are correct and when the stimulus is ambiguous. These papers are especially interesting in that original techniques of research are used and reference to other work is minimal. A novel technique is also employed by Lefkowitz, Blake & Mouton (82) in their field experiment on conformity by pedestrians to traffic signals. They show that nonconformity can be stimulated by having a well-dressed person violate the signal.

Deutsch & Gerard (28) approach the problem a bit differently from Mausner in their distinction between "normative" and "informational" influence on individual judgments. They point out that most studies have employed collections of subjects with no enduring relations to one another and that pressures to conform under such circumstances must be essentially informational. Only when an actual group exists can normative influences come into play. Experimentally varying degree of "groupness," degree of private or public commitment to own judgment, and ambiguity of the stimulus, they find greater conformity when there is greater "groupness," less commitment, and ambiguous stimuli combined with little commitment. Kidd & Campbell (71) show that experience of success on a group task can increase conformity in groups.

That commitment to one's initial judgment heightens resistance to social influence is questioned by Fisher, Rubinstein & Freeman (38) in an important and original paper. They set up a continuous interaction situation in which the subject commits himself on a number of successive judgments and found that subjects are reluctant to change a response already made, but they also found that subjects attempt to anticipate the social influence in making their next committal response. The implications of this study for all conformity research using a series of judgments will be considerable, for each judgment cannot be considered independent of the preceding ones. The need, however, to distinguish between "commitment" and "decision" is indicated in a study by Bennett (8). She isolated and independently varied four components of "group decision" as employed by Lewin (83). These are: (a) group discussion, (b) individual decision to perform a specified action, (c) degree of publicness of the commitment to the decision, and (d) degree of consensus in the group. The results indicate no difference in tendency to carry out the advocated behavior between group discussion and lecture or between public and private commitment, but the making of an explicit decision to act in a setting of high group consensus does produce a tendency to act comparable to that reported by Lewin.

Studies of social pressure and conformity have tended to stress the way in which such pressures lead to uniformity of behavior. That uniformity is not necessarily the outcome of conformity is demonstrated in a careful investigation by Hall (52). He has shown that members of bomber crews develop norms concerning the proper behavior of the aircraft commander, which presumably are different from those of occupants of other positions. Further-

more, the strength of these norms increases with crew cohesiveness, and the behavior of the commander is more similar to the norms the more he wants to remain with the crew. Extension of the work on conformity and member roles to discussion groups should be facilitated by a new technique for determining norms and roles developed by Bates & Cloyd (6).

That a rumor will spread widely through a group having a state of cognitive unclarity about an important issue which is common to most members of the group is demonstrated by Schachter & Burdick (118) in a neat field experiment. They also report remarkably little distortion of a planted rumor as it passes through the group. It would appear from this and other studies in "natural" settings that distortions found by use of the technique of serial reproduction may rarely occur in spontaneously communicated rumors.

SOCIAL INTERACTION AND GROUP DYNAMICS

Although research on group dynamics continued to be active this year, creative ingenuity was confined largely to methodology. An exception to this generalization is the imaginative paper of Burns (14) on cliques and cabals. In the firm he studied, older men formed cliques to achieve compensatory satisfactions for lack of advancement, and younger men formed cabals to improve by illegitimate means their chances of success. The norms, values, styles of communication, and inevitable impermanence of these two types of groups are made more understandable by this analysis.

Task requirements and personal needs.—A surprisingly large number of studies have converged this year on the conflict between requirements of performing a group task and personal needs of group members. Wispé (152) indicates that among insurance salesmen achievement is attained at the expense of peer-group affection. The work of Strodtbeck & Mann (133) on mock juries and that of Slater (127) on discussion groups suggest that such conflict may result in a specialization of two roles, one concerned with the task and the other with social-emotional relations. Juries composed of both men and women resemble the families observed by Strodtbeck (132) in which men tend to be task specialists and women social-emotional specialists. The relative importance, however, of power and acceptance in determining who will be leader depends, according to Wolman (154), upon the motivational objectives of the members of the group. An earlier paper by Lanzetta *et al.* (77) showed that a high level of threat will reduce task-directed behavior and increase concern with group acceptance. Lanzetta *et al.* (78) now report that group members receiving "anxiety-reducing" medication behave under threat as if they were having an active, nonaggressive "good time" with no special concern for task performance. It would be interesting to know whether task leaders emerge under such conditions.

Conflict between task requirements and social-emotional needs is also suggested by Torrance (141) in his comparisons of reactions of effective and ineffective bomber crews to two Group Projection Sketches developed by Henry & Guetzkow (56). The results are consistent with the view that energy

spent in maintaining a pleasant psychegroup is diverted away from socio-group task functioning, but they are also consistent with the assumption that more cohesive groups are freer to express hostility, a result produced experimentally by Pepitone & Reichling (104) in comparing hostile reactions to frustration of high and low cohesive groups.

Group performance.—Morse & Reimer (100) measured changes in productivity and morale after altering the decision-making structure of four parallel divisions of a large business organization. An Autonomy program, which increased participation of rank-and-file employees in decision making, raised employee satisfaction and productivity. A Hierarchical program, which increased the decision-making power of upper management, lowered employee satisfaction but raised productivity. The authors believe that measured productivity increased more in the Hierarchical than Autonomous program because their measure did not reflect adequately many morale-related efficiencies which might be expected in the Autonomy program. Lawrence & Smith (79) experimentally introduced a procedure of group goal-setting and one of group discussion and found that the former resulted in significantly larger increases in production than the latter.

Some determinants of the accuracy of group judgments were investigated by Ziller (157) who required aircrews to make group judgments of the number of dots on a card. Three experimental conditions were used: (a) preceding the group discussion, a census of estimates was taken in a sequence according to the power structure of the group, (b) the census was taken counter to the power structure, and (c) no prediscussion census was taken. Final private estimates were taken in all conditions. More accurate group judgments were reached when the initial census revealed a wider range of judgments, and group members tended to conform privately more to a superior group decision than to an inferior one. The results suggest that conducting a prediscussion census heightens accuracy and that conducting this census counter to the power structure yields a wider range of judgments.

Comparisons of the performance of groups with that of individuals continue to be made. Yuker (155) compared individual and group recall of a story read to groups of subjects and found that group recall was superior to average initial individual recall, initial individual recall of the persons with the best memories, and average final individual recall. That the specific nature of the task significantly affects success in predicting group performance from knowledge of prior individual performance is reported by Comrey & Staats (22) who achieved considerably greater success in predicting performance on a crossword puzzle than on a manual task. A methodological warning in comparing group with individual performance is advanced by Lorge *et al.* (84) who found that groups of ROTC men performed better than individuals on a complex field problem but that written reports from the groups were inferior to their rated performance whereas written reports from individuals were superior to rated performance.

Group assembly.—Several papers have focussed on the ways in which the

composition of a group affects its functioning. Analysis of variance was employed effectively by Rosenberg, Erlick & Berkowitz (113) to demonstrate that different combinations of the same individuals significantly affect the performance of three-man groups. While this study shows the existence of an assembly effect, it leaves unanswered the more difficult question of what contributes to the effect. Schutz (119) provides one approach to answering this question. He reasons that people will be compatible if they agree on the relative importance of power relations and love relations in making decisions. Results of an experiment show that compatible groups, thus defined, perform better than incompatible ones but that characteristics of the task and task-context are also important determinants since compatibility makes little difference where pressure is low and need for cooperation is slight. Cohen (21) demonstrates that a psychoanalytic approach to group assembly may be fruitful. Using the Blacky Test of Blum (11) to measure psychosexual disturbance and type of defense, he assembled pairs of subjects so as to produce various combinations of these. The most disruptive combination was one in which both subjects share the same psychosexual disturbance and both employ projection as a defense. In general it appears that pairs of similar defenders produce decreasingly negative interactions as the defenses range from projection, regression, reaction formation, to avoidance. Decreasing negative interaction also occurs as the area of psychosexual disturbance ranges from sibling rivalry, castration anxiety, oral sadism, anal expulsiveness, to oedipal conflict.

Still another approach is suggested by the theory of complementary needs in mate selection advanced by Winch (150). This theory holds that maximal need gratification will occur between two people whose need patterns are different rather than similar. Winch (151) concludes that while all of his evidence does not support the theory none of it supports the contrary theory of homogamous mate selection. Kelly (68), however, reports nearly all positive correlations among measures of spouses. A reconciliation of Kelly and Winch may be possible since none of Kelly's measures deals directly with needs. Additional support for Winch's theory is provided by Ktsanes (73) who did a Q-type factor analysis of responses of spouses to a need-interview, and by Gross (50) who studied spontaneously formed groups in the Air Force. The latter distinguishes between symbiotic ties in which each person has something needed to offer the other and consensual ties in which persons are held together by agreement and concludes that small groups of men get along with each other better if they are united symbiotically than they do if they are united by consensus.

Methods.—A systematic comparison of the methods of observing interaction developed by Bales (4) and by Carter *et al.* (17) was carried out in Finland by Kuusela (74) who obtained significantly better reliabilities with the Bales method. Even though Carter's method contains many more categories, obtained frequencies piled up in so few categories that the larger number of categories added little. Landsberger (75, 76) succeeded in utilizing Bales'

method in analyzing transcribed recordings of labor-management mediation cases and obtained some suggestive results. The possibility that the "content-free" recording of the Interaction Chronograph may be useful in studying the behavior of patients in a standardized psychiatric interview was explored by Saslow, Matarazzo & Guze (117) who found excellent inter-interviewer reliability and sensitive responsiveness to planned variation in interviewer behavior.

Two rather extreme attempts to standardize laboratory conditions have been reported. Kaiser & Blake (62) led subjects to believe that they were members of a five-person group though they never saw or heard the other subjects. Sidowski, Wyckoff & Tabor (126) designed a "minimal social situation" in which two subjects pushed buttons and received reward of shock depending upon the behavior of the other. Neither knew that the other was present. Subjects of Kaiser & Blake erroneously believed that others were present, while those of Sidowski, Wyckoff & Tabor erroneously believed that they were alone. The real merit of either deception is yet to be demonstrated.

Weiss & Jacobson (146) have devised a way of characterizing the structure of complex organizations by asking members of an organization to designate the people with whom they work most closely. The data are then put in matrix form and manipulated so as to reveal work groups, liaison persons, and contact between work groups. Sagi, Olmsted & Atelsek (115) were able to predict maintenance of group membership by measuring involvement in group and sociometric status. A classroom demonstration of the effects of social power upon group acceptance has been developed by Zander & Cohen (156).

Leadership.—A most unusual attack on the problem of leadership is launched by Rainio (107) who develops an extensive theory in the field-theoretical concepts of Lewin and tests it by factor-analyzing the responses of over 700 Finnish industrial supervisors to 28 remarkably diverse tests. In keeping with his theory, the factor analysis reveals four factors: (a) general intelligence, (b) nonsuggestibility, (c) stability, and (d) activeness. The multiple correlation of the test series measuring the first three factors with leadership ratings given by superiors is .57 and with ratings given by peers is .69. One stimulating aspect of this monograph is that it cannot be placed neatly within the customary American framework. It is not properly classified as a "trait" or "situationist" approach, nor does it fit into any of the various American philosophies of leadership. Perhaps the most dramatic contrast is to place it alongside the treatment of "group-centered leadership" advanced by Gordon (49). Rainio's nonsuggestible, stable, and active leader is surely different from Gordon's group-centered leader who "tries to facilitate the transference of control and authority to the group as a whole and to become more and more a group member and less and less the group leader."

Fiedler (36) has now extended his earlier work (35) on informal leadership of basketball and surveying teams to formal leadership of bomber and tank crews. The critical measure in these studies is the ASo score (assumed sim-

ilarity between opposites) which is obtained by asking the subject to fill out a personality questionnaire under instructions to predict the responses of a person whom he likes best and one with whom he finds it most difficult to cooperate. The more different these two sets of responses, the lower is the ASo score. In the earlier work it was found that those groups which chose a leader with a low ASo score tended to be the more effective ones. The data from bomber and tank crews now require a reformulation of these generalizations because the ASo score of the formal leader seems to have no effect unless he is accepted by the crew and important consequences derive from relations between the leader and the keymen or specialists who perform operations most crucial for group performance. Effective groups, it now appears, tend to have either of two kinds of accepted leaders: (a) those with low ASo scores who have a close relationship with their keymen, or (b) those with high ASo scores who have a distant relationship with their keymen. Fiedler is inclined to interpret the ASo score as indicating a general predisposition toward being close or distant toward people (low ASo reflecting distance). It would then appear that a leader who is generally distant must temper this characteristic by being close to his keymen and one who is generally close must maintain distance with his keymen. While this interpretation is intriguing, it would be desirable to obtain more direct evidence concerning the meaning of ASo scores. One is struck, for example, by the similarity between a high ASo score and high "role-taking aptitude" as measured by Sarbin & Jones (116), and these authors report that their measure correlates with "ego strength." If Fiedler's ASo score actually measures "ego strength," the interpretation of his results would be quite different.

The emergence in decision-making conferences of "real" leaders other than the chairman was found by Crockett (25) to occur when the chairman performed relatively few acts of goal setting, development seeking, solution proposing, and problem proposing, and when the committee displayed cliques or low motivational congruence among members. Apparently similar results are reported by Shaw & Gilchrist (124) from their study of leaderless groups of college students. Emergent leaders here were found to initiate more attempts at organization and more acts of giving information. Goldberg (46) has repeated the work of Leavitt (81) on the effects of communication networks upon choice of leaders but with a different kind of group task. As in Leavitt's experiment, persons occupying more central positions received more leadership nominations.

AUTHORITARIANISM

The impact on social psychology of *The Authoritarian Personality*, published in 1950 by Adorno *et al.* (1), is still clearly discernible. The F-scale has become one of the more popular tests, but questions remain about the nature and correlates of authoritarianism.

The nature of authoritarianism.—That the F-scale is not unidimensional has been suggested by a number of investigators. Bass (5) constructed a G-

scale made up of items in opposition to those of the F-scale. He defined "acquiescence" as a tendency to support items on both scales and "authoritarianism" as a tendency to support F-scale but to disagree with G-scale items. He concludes that among college students acquiescence accounts for 59 per cent of the variance on the F-scale while authoritarianism accounts for only 22 per cent. In 1951 Leo Srole reported in a still unpublished paper that his scale of "anomie" was better related to prejudice than the F-scale, especially at lower levels of education. This study has now been repeated by Roberts & Rokeach (108). Their results, while supporting Srole's contention that both authoritarianism and anomie are associated with prejudice, are more favorable to the original work on authoritarianism in that prejudice is more closely related to authoritarianism than anomie, and status has no effects. Perlmutter (106) concludes from an extension of his earlier work on xenophilia (105) that the High Authoritarian-High Xenophile favors authority but seeks to escape it while the Low Authoritarian-High Xenophile is anti-authoritarian rather than nonauthoritarian.

Further support for the original conception of the authoritarian personality is provided by Meer (97) who shows that dreams of authoritarians are more aggressive and less friendly toward outgroup than toward ingroup characters while nonauthoritarians reveal neither of these differences. That authoritarians display a stereotype of others as similar to themselves was previously reported by Scodel & Mussen (121), but Crutchfield (27) suggested that their results might also mean that nonauthoritarians hold a stereotype that others are different from themselves. Scodel & Freedman (120) now conclude from another experiment that social perceptions of low authoritarians are influenced to some degree by an "other stereotype." It appears from all these studies that the low authoritarians, at least, are not a homogeneous group. This, or other reasons, may account for the finding of McCandless & Holloway (92) that six operational definitions of "intolerance of ambiguity" are not related to prejudice among fourth and fifth grade children.

Correlates of authoritarianism.—Confirmation of the finding of Frenkel-Brunswick (42), that parents of highly ethnocentric-authoritarian children tend to use more harsh and rigid forms of discipline than parents of less authoritarian children, is reported by Lyle & Levitt (89). They also find that high authoritarian children themselves tend to be more punitive when punitiveness can be expressed without fear of retaliation. It should be noted, however, that parental punitiveness is measured in this study by reports of the children. If highly ethnocentric-authoritarian children tend to see all social objects as punitive, then the conclusion that their parents' punitive behavior was the cause of their authoritarianism could hardly be drawn from these data. That authoritarian reactions are defensive in nature is also suggested by the conclusion of Wispé & Lloyd (153) that among life insurance salesmen personal insecurity heightens the desire for "structured personal interaction."

Does the High Authoritarian tend more than the Low to accept others of

higher status and power? Medalia (96) finds that among members of radar squadrons more Highs than Lows do idealize and accept their formally designated leader. The Highs also express a more favorable attitude than the Lows toward re-enlistment in the Air Force. However, Thibaut & Riecken (135) report no difference between Highs and Lows in their initial acceptance of a person of higher military rank, but when this person behaves unfavorably Highs tend to reject him less than do Lows. On the other hand, if this person is of lower military rank, Highs come to reject him more than do Lows. Masling, Greer & Gilmore (90) find that among infantry riflemen and Naval Recruits Lows tend generally to make more positive sociometric choices than Highs and to receive more. This last conclusion must be qualified among official leaders, who receive a large number of choices regardless of their own authoritarian dispositions.

SOCIAL STRUCTURE AND SOCIALIZATION

An interesting approach to the study of cultural determinants of evaluations of personality is described by Rosen (112) who had subjects complete a personality inventory three times so as to reveal their perceptions of actual self, of their ideals for self, and of society's ideals for the self. Analysis of the variance among subjects on each of these sets of responses reveals a fairly homogeneous norm for personal standards, a more heterogeneous perception of social standards, and considerable divergence of actual personalities. The situation for this group of subjects might be described as one in which they believe themselves to be deviating from social pressures and conforming to their own standards, but these personal standards are themselves rendered remarkably homogeneous within the group by some unrecognized process.

The semantic differential method of Osgood (101) was used by Lazowick (80) to study the "identification" of college students with their parents. Measuring identification by the degree of similarity between subject and model as revealed by Osgood's method, Lazowick finds that normal male subjects tend to identify more with both parents than do neurotics and that the degree of anxiety in a male child can be predicted from the degree of "semantic harmony" between his parents. Comparable analyses for females gave strikingly less significant results. Helper (55) also reports marked sex differences in the effects of children's modelling of their self-concept after their parents. For boys, a high degree of parental reward for similarity to father and an actual high degree of modelling after the father are associated with high peer status. For girls, high parental reward for similarity to mother is associated with low peer status, and actual similarity is unrelated to peer status. A conflict between ideal and actual roles for the two sexes in marriage is revealed, according to Saenger (114), by an analysis of comic strips. Unmarried males in the comic strips are stronger, more powerful and vigorous than females, but married males are less mature, indecisive, and under the domination of females. In the world of comic strips, men lose strength and height after marriage, as well as most of the arguments.

The techniques of McClelland *et al.* (94) for measuring the achievement motive have been used to relate this motive to social class and family child-rearing practices. Douvan (29) studied achievement responses of middle-class and working-class high school students following monetary and symbolic deprivation. She concludes that middle-class subjects have a more generalized achievement motivation since they score high following both kinds of deprivation whereas working-class subjects score high only after monetary deprivation. An important contribution to research on achievement is made by Rosen (111) who demonstrates that social classes differ not only in achievement motivation but also in achievement values. He shows further that high motivation, but not high values, leads to higher grades in school while high values, but not high motivation, lead to the decision to go to college. When both motivation and value are held constant, there is no relation between social class and achievement or aspiration. That level of occupational aspiration is related among middle-class college students to feelings of familial rejection (high rejection associated with high aspiration) is reported by Dynes, Clarke & Dinitz (31), although only a small part of the variance in occupational aspiration is accounted for by this single variable. We note in passing the finding of Alper, Blane & Abrams (2) that middle-class children are subjected to earlier bowel training than lower-class children and that they enjoy finger painting less because they dislike getting dirty.

It may be appropriate to close by citing this reviewer's favorite contribution of the year. In a bold, yet modest, attempt to employ psychological theory to account for the economic development of countries, McClelland (93) maps out a way of testing the classic thesis of Weber (145) concerning the nature and characterological consequences of the Protestant Reformation. McClelland restates the thesis in the following way: (a) Protestantism (self-reliance values) leads to (b) independence training of children by parents, which leads to (c) high need Achievement in children, which leads to (d) economic and technological development. He then cites evidence to show that Protestants favor earlier independence training than do Catholics, that mothers who advocate early independence have children with high need Achievement, and that students with high need Achievement are more interested in business occupations than students with low need Achievement. All of this is made more plausible by an analysis which shows that the degree of Protestantism of a country is significantly associated with its level of consumption of electric energy. Social psychology could use more of this sort of daring.

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INDUSTRIAL PSYCHOLOGY¹

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INTRODUCTION

In their earlier reviews of this subject, Kendall (63) and Wallace & Weitz (122) have accurately described industrial psychology as heterogeneous and lacking in technical-scientific unity. However, the demarcation of industrial psychology is after all essentially a matter of location rather than of subject matter. Its variables and methods are drawn mainly from psychology's major substantive divisions, including social, differential, experimental, and clinical. Accordingly, the unification of industrial psychology cannot precede that of psychology itself.

Nevertheless, a general structure to the field is gradually evolving, defined primarily by the variables with which it is concerned. The dependent variables are basically two: performance or satisfaction or both on the part of personnel.

There are roughly three broad types of independent variables: personal characteristics of job incumbents in relation to job requirements, conditions and practices which may affect the ability of people to work, and conditions and practices which may affect the motivations and attitudes of people toward work. The first of these categories is represented mainly by studies on personnel selection and training; the second includes equipment design and working conditions and methods; the third is illustrated by studies of incentives, intra-group relations, and labor-management relations. Investigations of patterns of supervisory behavior and of systems of organization often spill, somewhat uncertainly, across the last two categories.

The summary of current literature presented below is organized in terms of groupings of independent variables like these. The field of consumer research, which has a different set of dependent and independent variables, is also discussed.

The framework outlined above is obviously both gross and approximate. It is also, of course, tentative. However, to the extent that it comprises most of what is now being done in industrial psychology, it indicates that the field is healthily discarding an *ad hoc* agenda in favor of pursuing systematically a limited number of broad and not unrelated issues. This augurs well for the long-awaited development of comprehensive theories.

However, there are still disquieting symptoms of opportunism and of unimaginativeness in what is being done. Heron's (48) remarks are well worth heeding, with respect to the need for a better synthesis among scientific, technological, and professional considerations. He calls for sensitivity to new and significant variables, for flexibility in reformulating problems on

¹ This review is based on literature received through April 15, 1956.

the basis of new knowledge, and for use of established principles in relating theory to practical problems. He feels rightly that improvements along these lines will add both to the scientific status of industrial psychology and to its value and acceptability in industry.

With this general preview of coverage and of strengths and weaknesses, let us proceed now to an examination of the year's literature, as it appeared for the most part in standard American and British psychological sources. The work of a few nonpsychologists was included, particularly in connection with topics (such as organizational policies and practices) about which psychologists published little; some continental European work was covered but, in general, the reviewer's shortage of time, space, resources, and literacy conspired to dilute its representation.

CRITERIA AND DEPENDENT VARIABLES

As Blum states in the recent revision of his excellent textbook (8), industrial psychology has two general objectives: enhancement of satisfaction on the part of industry's participants and improvement of effectiveness of performance. Accordingly, investigations in industrial psychology typically employ one or both of two classes of dependent variables, measures of satisfaction and of performance. Although, as will be discussed later, an increasing amount of research is being directed at defining the relationship between these two types of measures, their degree of interdependence is still unclear.

The term "criteria" has been traditionally applied to dependent variables when used for the validation of psychometric instruments. Recognizing that the exactness of their knowledge is limited by the relevance and metric accuracy of the dependent variables and criteria employed, industrial psychologists have been much concerned with this subject. Even now, Division 14 of the American Psychological Association has a project under way regarding the use and improvement of criteria.

The literature of the past year reflects continuing attention to the problems of criteria and dependent variables. For example, both Ghiselli (33) and Sluckin (104) review the problem of how to treat several separate criteria of performance pertinent to the same job. Ghiselli points out, too, that longitudinal studies may be needed to cope with the possibility that factors making for job success may differ in relative importance at various stages of job experience. He also suggests that people in the same job may perform in different fashions and that this criterion dimensionality of the individual may be studied by *Q*-technique. The reviewer is not sure but what this last point is fundamentally a reiteration of the problem of criterion multidimensionality raised in the first problem, above; however, a transposed analysis would, to be sure, be another way of defining the performance dimensions. This general area is growing more important as criterion measurement becomes increasingly refined.

A highly significant article has appeared on the concept of constructs and

construct validity [Cronbach & Meehl (22)]. A construct may be defined as a postulated attribute which is not operationally defined and for which only fragmentary or imperfect criteria exist. Many of the "variables" with which industrial psychologists are concerned are constructs of this type, including especially those relating to personality, interest, and attitude. The understanding and validation of a construct require the establishment of a network of interrelationships among measurements of it together with many other observable variables. The validity of a construct therefore cannot ordinarily be expressed in terms of a correlation coefficient between two variables; the closest approximations to construct validity coefficients may be of the nature of factor loadings.

To the industrial psychologist, this concept seems to have greatest pertinence along two lines: (a) in the understanding of complex phenomena that at present cannot be adequately studied on the basis of simple two-variable designs (research on motivation and morale being a prominent example of this application) and (b) as an intermediate step in the development of more predictive tests and more predictable criteria. Construct validity should not be regarded as a substitute for predictive validity, since industrial psychologists must always be concerned with the performance and satisfaction consequences of individual differences in test scores.

Another type of difficulty frequently encountered in validating and setting standards on tests is the lack of comparable performance data for a sizeable number of incumbents of a given job. Lawshe & Steinberg (71) propose that this problem be circumvented by using as a criterion the extent to which a job involves operations judged to be related to the trait tested. A test has what they term "synthetic validity" if it distinguishes, on the average, between incumbents of jobs (regardless of titles) which are and jobs which are not judged to involve the tested trait to a considerable extent. In a study of clerical workers, the authors found that four tests did show such "validity," whereas three did not. This is a refined and interesting application of the concept of interoccupational differences in test performance as a gauge of validity. However, the reviewer feels that the term is better reserved for evidence that differences in test performance are correlated with differences of performance in a given job. Incidentally, both this study and one by Trattner, Fine & Kubis (118) show that trained personnel can fairly successfully predict interoccupational differences in average test scores on the basis of job-description data.

Both objective and subjective (or judgmental) measures have traditionally been used for each of our two basic types of dependent variables, i.e., satisfaction and performance. It is a pleasure to report that there seems to be a trend toward the increased use of objective measures. This is appearing to some extent in the field of selection but even more so in studies of training, of attitude and motivation, and of supervision and organization. Specific examples of various criterion measures will be furnished later in discussions of the independent variables in connection with which they have been used.

A related noteworthy trend is the growing concern with evaluating and measuring outcomes of broad programs. Stone & Kendall (116), for example, have included in their book on personnel selection a chapter on evaluating effectiveness of personnel programs. They recommend regular audits, covering policies, practices, and results. These last include productivity data, absenteeism, waste, and training time required, as well as attitude surveys. Luck (74) has written an entire book on the same subject; while it leaves much to be desired in its technical aspects, the book's general message and purpose are commendable.

The system of reviews whereby the Army undertakes to gauge the quality of its civilian personnel management has been described by Willey (126). Among other steps, statistical reports are compiled at each installation regarding quits, supervisory ratio, skills training activities, suggestions submitted, promotions, AWOL, and similar indications. Both Luck and Willey fail to differentiate clearly between outcome or criterion variables and input or independent variables. For example, turnover and training are mentioned in the same breath. This confusion creates the risk of evaluation partly on the basis of the face-validity of practices which themselves need evaluation.

The development of suitable criteria for measuring the human results of organizational policies and practices represents a fertile field for psychology's contribution toward a science of management.

EMPLOYEE ATTITUDES

General.—This topic has grown to the point where it is probably the most active one in current industrial psychology. Four textbooks devoted largely to it have appeared during the past year [Blum (8); Haire (39); Smith (105); Stagner (111)]. Herzberg *et al.* (50 to 56) have comprehensively surveyed the literature dealing with it prior to 1955. A considerable quantity of new research has also been published, much of it of good quality.

There have been several articles on questionnaire methodology. Kirchner & Uphoff (64) reported a study verifying previous evidence to the effect that employee attitude findings are not affected by the order of presentation of questionnaire items, whether in topical groups or in mixed order.

Rosen & Rosen (97) propose that each issue or topic in a questionnaire survey be covered by three items: one which permits a statement of the employees' standards regarding the practice or condition at issue, one descriptive of the employees' perceptions of the current situation, and one expressive of the degree to which employees are satisfied with the situation. They believe that this provides a better basis for interpreting the nature and bases of the attitudes expressed.

Items of the "perception" type were prominent in the questionnaire studies of factors related to organizational effectiveness conducted by the Southern California group (18, 19, 20). Reports were published during the year concerning further analyses of three of the questionnaires. The first forms of the questionnaires each consisted of approximately 20 sets of about 5 items each, the groupings being effected on the basis of item analysis.

Wherry-Gaylord iterative analyses reduced the number of sets and items, and improved the independence and internal consistencies of the resulting dimensions. A centroid factor analysis of the 14 dimensions in the questionnaire for district rangers produced the following group factors: efficient management, consultative supervision, familiarity with subordinates, and forceful supervision (17). A similar analysis of the 16 dimensions in the questionnaire for aircraft workers reproduced the first three of the above factors, plus a new fourth factor: group cohesion (57). A factor analysis of the 19 dimensions of the aircraft supervisors' questionnaire yielded eight factors: again the first three listed above, plus communication, counseling, paternalism, irresolute supervision, and pressureful production (58). Since the dimensions were nearly independent, the factors reported above are indicative of areas of residual overlap and are more in the nature of second-order factors. It should be noted that not all of the dimensions were originally found to be correlated with criteria of performance, so that the factors should not be interpreted at this point as necessarily representing ingredients of organizational effectiveness.

Approaches to studying employee attitudes.—In their chapter in the 1955 *Annual Review of Psychology*, Wallace & Weitz (122) point out that job satisfaction may be regarded either as an end in itself (i.e., a criterion) or as a predictor of performance. Although it has not been clearly stated, a third position may also be detected in current research and thinking on the subject: that job satisfaction and attitudes refer to a motivational construct or set of constructs not adequately measured by any given technique. This position entails the investigation of a network of relationships among attitude and other variables, such as was described above in the discussion of construct validity (22). In the present instance, some of these other variables may be conditions or practices, while others may be measures of job performance.

Since current research reflects all three of the above approaches, this section deals first with employee attitude and satisfaction as a dependent variable, then with performance as a dependent variable in attitudinal research, and finally with the interrelations among these two and other "observables."

Employee attitude as a dependent variable.—Typical of this approach is a study reported by Vollmer & Kinney (121). In terms of questionnaire averages, more job satisfaction exists among older employees and among those with less education (in the latter instance, even when job level was held constant).

Krugman (67) compares job satisfactions of technical personnel with those of production, maintenance, clerical, and supervisory personnel. He reports that the order of satisfaction with various job aspects is similar among these several groups. His interpretation is that scientific personnel react to work situations in much the same way as others and are therefore not a special breed. This, however, should not be taken to gainsay differences in the specific features of how their jobs are to be organized and supervised.

Perry & Mahoney (92) surveyed employee attitudes toward their com-

pany and tested their information about the content of communications from management. They report that there is no correlation between attitude and information scores of individuals. This should serve as a caution signal to those faddists whose prescription for morale problems is communication, no matter what or how.

Smith (106) had 72 female mill-workers express their feelings of job boredom via a questionnaire. They also provided information concerning personal characteristics. The following characteristics are found to be negatively related to reports of boredom: age, preference for regularity in daily routine, preference for inactive ways of spending leisure time, and satisfaction with home and factory life.

An interview study by Marriott & Denerley (78) in three British factories shows that satisfaction with wages is found to parallel wage levels; satisfaction with shift is greater on the part of employees working a regular day-shift than those on rotating shifts; those working on conveyor jobs rate their work as less satisfying than those on other jobs; attitudes toward the group payment system are more favorable in the case of one factory that had a simple and understandable system, in contrast to those in another factory with an involved one.

Taken collectively, the foregoing studies illustrate how job satisfaction and attitudes may vary with attributes of the employees or of their work.

Employee performance as a dependent variable.—Employee performance is used, of course, as a dependent variable in a wider variety of research than that relating to employee attitudes and satisfaction. However, there is a clear current of research in which performance measures are studied in relation to conditions and practices in the work-situation, with employee attitudes and motivation as mediating constructs even though they are not otherwise measured.

An example in which the construct is explicit is a study by Hill & Trist (60). They take the position that absence phenomena, including those occasioned by accidents, reflect the quality of the relation of employees to the company, and that absences function as a means of withdrawing temporarily from unsatisfactory situations. The incidence of absences was found in their study to reach a peak during the second half-year of service and to subside to a stable level after about 2.5 years; there was a progressive trend for "sanctioned" forms of absence to be substituted for "unsanctioned" forms. The incidence of accidents also declines. These results are interpreted as an indication of improved adjustment with time on the part of employees.

Another example of this type of interpretation is furnished in a report of Fleishman, Harris & Burt (28). Finding that absenteeism among employees is greater when supervisors show less consideration, and grievances are higher under supervisors who are more disposed to initiate and impose structure, the authors interpret these results in terms of the poorer attitudes that employees have toward such supervisors.

Merrihue & Katzell (85) similarly express the hypothesis that an attitude factor is reflected in various employee behaviors, including absences, turn-

over, nonparticipation in benefit programs, suggestions, injuries, actions incurring disciplinary suspensions, grievances, and work-stoppages. Their factor analyses of these variables show that a general factor is present among them. They derive a general factor score for work groups or plants, terming the score the Employee Relations Index or ERI (higher ERI's being "better," i.e., representing fewer absences, grievances, etc.). ERI is often but not invariably, found to correlate positively with evidences of performance effectiveness, such as production figures and managers' ratings of work groups; plant ERI's are found to correlate about .4 with profitability data, in a sample of 17 plants. Merrihue & Katzell propose that ERI may be used as a dependent variable. In some exploratory work along these lines, it has been found that ERI's of work groups are negatively correlated with number of employees in the group and positively correlated with average age and length of service. This is consistent with findings of studies of employee attitude conducted by other means. With respect to impact of managerial policies, the authors report that plant ERI's are significantly higher when the work force is composed largely of members of the same sex, when employment level is not expanding, and when a relatively large proportion of employees work in teams.

Odiorne (89) made observations in two manufacturing departments which differed markedly in percentage of machine down-time. In the department in which machines were more often out of order, there was greater incidence of absenteeism, tardiness, quits, grievances, and observed arguments among employees. While this was not a technically elegant study, its findings are suggestive. The author's interpretation is that the presence of poor equipment causes poor morale. However, the reverse may also be true, or the two may both be expressions of some third condition, such as poor supervision. In any event, a number of performance variables seem to be interrelated here.

Production is used to reflect hypothesized changes in morale in Lawrence & Smith's (70) better controlled repetition of the well-known experiment on employee participation conducted at Harwood Manufacturing Company (29). Control groups were used which were drawn into group discussion about the work but were not involved in setting production goals; the experimental groups did both. The results show that all groups significantly improved their production as compared to the period prior to introduction of the discussions. However, the experimental groups improved their production significantly more than did the control groups.

What is still not clear to this reviewer is the extent to which such participation effects may be attributable to the focussing of the employees' attention on the importance of production, accompanied by official information on production levels (i.e., knowledge of results). Although more investigation will be needed before we fully understand what is happening in this type of situation, Lawrence & Smith deserve our thanks for making a good start in this direction.

Interrelations among attitudes, performance, and practices.—During 1955,

two reviews were published of previous research on the relations between employee performance and attitude measurements. One of the series of reports prepared by Herzberg *et al.* (52) deals largely with this subject. They find that, among the studies in which performance was compared with favorability of job attitudes, 54 per cent reported a positive relationship, 35 per cent no relationship, and 11 per cent negative relationship. Most of the relationships were low. They also conclude that there is more unequivocal evidence of relationship (negative) between job attitudes and both turnover and absenteeism, with some data also supporting the same trend as regards accidents and psychosomatic illnesses. In general, they conclude that the belief that "positive job attitudes are a tremendous asset to industry is supported by much of the experimental evidence now available."

Brayfield & Crockett (10), on the contrary, infer in their review that "there is little evidence in the available literature that employee attitudes . . . bear any simple . . . or . . . appreciable relationship to performance on the job." It should be noted that their definition of "performance on the job" excludes absenteeism, accidents, turnover, etc. (they do infer that attitudes are related to absenteeism and turnover). This is one source of difference in the tone of the two conclusions. Another reason for the difference is that the two studies do not cover exactly the same literature. Moreover, Herzberg *et al.* are more receptive to suggestive findings, whereas Brayfield & Crockett slight anything not statistically significant. (Whereas the latter position is, of course, more "correct," it also entails the danger of Type II errors regarding the null hypothesis, since most of the studies were based on small *N*'s because the unit measured was more often the group than the individual.) But perhaps the main reason for the disparity is that Brayfield & Crockett state their generalization prior to their consideration of the parameters involved in the relationships between attitudes and performance, whereas Herzberg *et al.* more appropriately take such influences into account in arriving at their over-all judgment. Both sets of authors make good suggestions on how improved methodology and research design may result in better clarification of these relationships.

For some obscure reason, Brayfield & Crockett felt compelled to moralize as follows: ". . . it is time to question the ethical and strategic merits of selling to industrial concerns an assumed relationship between employee attitudes and employee performance." This reviewer has encountered no evidence, either in the psychology literature of the past few years or in his personal contacts among industrial psychologists, that would justify this chastisement.

The past year's literature has several contributions to make toward clarification of the attitude-performance picture. Hersey (49) has analyzed what factory workers reported in interviews during periods when their production deviated from "normal." He finds four categories of factors associated with these deviations: worker's physical condition; conditions and events related to the plant, the work, and other employees; outside factors, including family and recreation; and climate. Workers' feelings and attitudes were

also gauged during the interviews; it was found that production is, on the average, only slightly above normal when feelings are happy but is about 7 per cent below normal during unhappy periods. Also, 60 per cent of 800 accidents were found to occur during periods of unhappy feelings; such feelings occurred only about 20 per cent of the time. In general, the work supports the thesis that employee feelings and behavior are correlated responses to environmental and personal conditions. It also underlines the multiple causality of variability in feeling and performance, thus reenforcing the view that there is no simple relationship between the two.

Härnqvist (40) studied attitudes of Swedish naval trainees by means of both questionnaires and five "non-verbal indicators" of adjustment to military life (e.g., training grades, sick calls, etc.). The average correlation among individual questionnaire items and these indicators was low, but this method of analysis may have obscured general relationships. It is of interest to note that the average intercorrelation among the nonverbal indicators was .17 (S.E. of zero r was .045).

The University of Southern California group has reported two more of its well-conceived studies on organizational effectiveness. In one of them (19) a questionnaire was administered to supervisors employed in 29 production departments; in the other (59) a similar questionnaire was administered to the 29 foremen in charge of these departments. In neither of the samples were there appreciable correlations between the questionnaire scores and objective criteria of work quality. Ratings of departmental effectiveness correlated significantly with a few of the dimensions in the questionnaire used with supervisors but not with the foremen's questionnaire. An objective criterion of productive efficiency was significantly correlated in the "expected" direction with about one-fourth of the dimensions in each questionnaire. In general, dimensions that show criterion correlations are those referring to confidence in the company, confidence and pride in subordinates, feelings of having adequate authority and influence, and, in the case of the supervisors, various human-relations skills of their superiors.

It is important to note that the productive efficiency criterion was the most predictable of the four. This was also the finding in a previous study of nonsupervisory employees in the company (18). Few of the past studies reviewed by Brayfield & Crockett and by Herzberg *et al.* used satisfactory production criteria, which may account for much of the ambiguity on the subject. It is also noteworthy that most of the predictive dimensions used in these Southern California studies are of the type that describe perceptions of practices or conditions intimately related to the job, especially those pertaining to supervisory methods; this contrasts with the general "job satisfaction" type of measurement often used in past investigations.

A substantial relationship between attitude measures and turnover has been revealed by several studies. In a particularly well-designed one Weitz & Nuckols (124) identified two matched groups of insurance agents, one of which had terminated its employment during the year following the administration of an attitude questionnaire whereas the other had maintained its

employment. As compared to the terminators, the survivors were found to have checked significantly more satisfaction items and fewer dissatisfaction items on the questionnaire. However, not all items were equally good predictors of turnover, since only 21 of the 140 distinguished between the two groups at the 5 per cent level or better. This study provides valuable confirmation for the field of attitude measurement of what we already know from ability measurement: item analysis against the criterion with which we are concerned will identify those items (and the underlying conditions) most relevant to that particular objective.

Webb & Hollander (123) found three different attitude measures to be predictors of voluntary separation from a naval air training program. The measures were as follows: a job attitude questionnaire containing 12 scored items, a peer nomination based on morale and interest for the program, and a self-rating of interest and enthusiasm. These three scores were obtained for each of 210 cadets, 16 of whom subsequently withdrew from the program. Fourteen of the terminators were below the median on the self-ratings and in the peer nominations; 10 were below the median on the questionnaire.

Sagi, Olmstead & Atelsek (101) studied withdrawal of members from college-student groups whose objectives consisted of the creation of products or services for the student body. Over a period of time, 63 of the group members remained with their groups, while 60 terminated their membership. The latter had significantly lower scores on both a personal-involvement attitude scale and a measure of sociometric status than did the former.

A highly significant experiment has been reported both by Likert (72) and by Morse & Reimer (87), the latter in more technical detail. It was designed to investigate the relationship between degree of employee participation in decision-making, their satisfactions, and their productivity. Two programs were installed among different clerical units in an industrial organization: an "autonomy" program, involving increased employee participation; and a "hierarchically-controlled" program, increasing the role of supervisors in decision-making. Employee attitudes and group productivity were measured both prior to the inauguration of the programs and after they had been in operation for a year. The following attitudes were found to increase significantly among employees in the autonomy program and to decrease under the other program: feelings of self-actualization, satisfaction with relations with supervisor, perception of supervisor as representing employee interests, and satisfaction with working for the company. Intrinsic job satisfaction did not change significantly. Productivity, based on cost of production, improved significantly under both programs and significantly more so in the hierarchically-controlled one. The improvements are viewed as resulting essentially from staff reductions, which were made by supervisors in the hierarchical program and which occurred mainly through turnover in the autonomy program.

Briefly, the view of this reviewer is that two sets of factors were operative in this experiment: a system of rewards and a structure of requirements. The autonomy program succeeded in improving the first, and, through its articu-

lation with the second, the latter also. The hierarchical program improved the second, but at the expense of the first. Better articulation of the two sets of factors in the autonomy program (e.g., through a group incentive system) may have led to even greater improvement in production. Conceivably, too, superior techniques of directive leadership may have led to high satisfaction in the hierarchically-controlled system. This study dramatically reveals how complex is the interaction between the work situation, human motivation and satisfaction, and performance.

This year's research has richly illustrated how job attitudes are affected by characteristics both of employees and of their jobs, and how performance is also subject to such influences. We seem to be coming more effectively to grips with the attitudinal correlates of the more simply determined behaviors such as turnover and absenteeism. Gradually, we are learning more of the conditions under which attitudes may or may not be related to productive performance; these include the nature of the attitudes measured, the adequacy of the performance criteria, and the extent to which behavior is regulated externally by the system. It is clear that not much is likely to be learned about the relationships between attitudes and productivity from simple two-variable designs.

ORGANIZATIONAL POLICIES AND PRACTICES

Studies like the preceding one call to our attention the fact that performance and satisfaction are profoundly affected by the institutional nature of the industrial enterprise, including especially its goals and values, the roles expected of its members, and its system of rewards and penalties. Appropriately, the literature of psychology and its kindred sciences is increasingly reflecting concern with human implications of organizational policies and practices.

Haire's new book (39) is perhaps the most explicit statement to date by a psychologist of these issues. Among other things, he points out that developmental trends in modern industry, such as bigness, work rationalization, and automation, require advance planning regarding the management of men if the changes are to be successful. This defines an important and challenging realm for future psychological research.

Ohmann (90) expresses the proposition that our industrial society has produced abundance without satisfaction. What is wrong, he feels, is a matter of goals and purposes. We should ask ourselves: How can production be justified if it destroys personality and human values both in the process of its manufacture and by its end use? Ohmann states that the answer is not that of working more efficiently so that one can finish his work day of drudgery and cultivate his soul on his own time. The key to the problem, he suggests, is to imbue work with a sense of special significance, although the techniques for doing this (what he terms "skyhooks") are difficult to define. Here, too, may be an area for important psychological work.

In the book previously mentioned, Haire (39) also expresses concern with much the same issue. His main recommendations regarding "skyhooks"

are that (a) managers develop greater sensitivity to human nature and be guided in their actions accordingly and (b) provisions be made for greater employee participation in decisions.

The Yale studies in assembly-line industry provided Turner (119) with the background for his remarks to the effect that the production results achieved by such organizational methods tend to disguise the fact that they set off a vicious cycle of negative attitudes which eventually runs counter to management's own interests, as well as that of employees. He advises that experiments be undertaken to introduce more individual and group participation in assembly-line work, and more variety and interest in jobs, so that organizational policies and methods may be guided by their results.

Whyte *et al.* (125) in their book attack a parallel problem of industry, financial incentive plans. They illustrate by case studies that the consequences of plans based on naive theories of motivation frequently defeat their purpose by inducing such worker reactions as restriction of output. The authors propose as a solution the creation of a co-operative system in which incentives, including those associated with finances, work satisfactions, inter- and intra-group relations, and union-management relations, operate in mutually consistent directions. Employee participation is seen as a key to development of such a system.

The proposal of employee participation as a feature of organizational policy has been recurrent in the literature cited above. We have gleaned a hint from the Michigan study (72, 87) to the effect that this proposal may confront industry with at least one problem, viz., the relative productivity of "permissive" and "directive" systems. Collier (14) suggests that the solution may be to adopt directive practices when the organization's purpose is primarily efficient production, and permissive practices if the purpose is creative. An article by Argyris (3) further highlights the same dilemma. He refers to evidence from the studies by his group to the effect that permissive industrial organizations seem to induce less tension, hostility, and frustration among the personnel; productivity, however, is not as high as employees report it can be. On the other hand, he theorizes that the usual directive organization, when it enforces its inherent principles of unity of direction, chain of command, span of control, and task specialization, operates to stifle growth and maturity on the part of its personnel, and to generate in them feelings of apathy, dependence, and failure. However, such an organization may fare better profit-wise and may better be able to provide rewards of income and security to its employees. Argyris' thesis is far from proven, but it is plausible. It points to the need for research on the impact of the organizational pattern on the individual, leading to new organizational principles.

Union-management relations.—Psychologists are showing increasing evidence of engaging themselves in this field. Two new textbooks on industrial psychology have included chapters on the subject [Blum (8); Smith (105)]. Stagner (111) has written a book which, more than any previous one, stresses the actual and potential contributions of psychology to the under-

standing and improvement of labor relations. He views union-management relations in terms of an accommodation process, characterized by conflict, co-operation, or both. The circumstances and conditions that may be regarded as fostering co-operative relations are described; here there are, of course, relatively few demonstrated facts, but Stagner's hypotheses and suggestions provide a sound point of departure for further exploration and investigation.

Whereas Stagner emphasizes co-operative accommodation as the road to industrial peace, McMurry (81) takes as his point of departure the proposition that labor-management strife is both inevitable and healthy. "The real question," he writes, "is not how to avoid or prevent labor warfare, but how to confine it within manageable bounds." This can be done effectively, he states, by the maintenance of a balance of power between management and the union. He therefore advises management to study systematically the employees' needs and feelings, through attitude surveys, exit interviews, etc., in order to be in a position to satisfy workers' material and emotional needs without having to be prodded into it by the union. Part of the reason for the apparent disparity between the positions expressed by Stagner and by McMurry is that they are at different levels; the latter's is essentially tactical rather than explanatory. The pursuit of all its ramifications and implications might well lead to Stagner's position.

Psychological science is also reaching the collective bargaining table. Landsberger (68) reports a study, by means of interaction process analysis, of the mediation conferences on 12 labor-management disputes. The 12 cases were ranked for success on the basis of the extent to which the issues were settled at the end of the session. The sessions were analyzed in terms of the degree to which each of the following trends occurred through the sessions, as previously observed in group problem-solving [Bales & Strodtbeck (5)]: decrease in the percentage of orientation interactions; maximization of the percentage of evaluation (opinion) interactions toward the middle of the session; increases in suggestions, in positive social-emotional interactions, and in negative social-emotional interactions. The extents to which these trends occurred in the 12 mediation sessions were found to correlate significantly with the success of the negotiations. Also, there was a significant inverse relationship between the extent to which hostility was expressed early in the session and the likelihood of success.

EXECUTIVE AND SUPERVISORY BEHAVIOR

The previous section on Employee Attitudes provided illustrations of the considerable extent to which those variables are affected by the behavior and techniques of executives and supervisors. The issues reviewed in the section on Organizational Policies and Procedures are also intimately linked with the behavior of leaders, for the nature of the organization in part both determines and is determined by such behavior. The present section will be devoted to literature that focusses primarily on the behavior patterns and functions of executive and supervisory personnel.

An article by Gray (36) suggests the crucial nature of this subject. A team of British social scientists had been assigned to study, by interview, the managerial methods by which the construction of a refinery in Britain had been completed ahead of schedule by British labor working under American management. The original object of the study was labor-management relations, partly because most of current relevant hypotheses are to be found in this field. However, the investigators' attention soon became drawn to intramanagement procedures and relations as the key to the achievement. As contrasted with the investigators' impression of usual British management, the management staff being studied was more optimistic and exhibited greater willingness to adapt and to try new things; there was more detailed advance planning, including anticipation of contingencies; unexpected difficulties elicited harder work, not excuses; and so on. Gray infers that, if British productivity is to be raised, the need for new systems, techniques, and attitudes on the part of management is even greater than the need for higher employee morale and effort.

A number of American psychologists have been, in one way or another, trying to define and describe the behavior of managers. Mandell (76) developed a check-list containing 80 items descriptive of supervisory behavior. When the check-list was filled out on first-level supervisors by their peers and superiors, 23 of the items were found to correlate with merit ratings received by these supervisors. A factor analysis of all 80 items revealed four factors of unstated independence: administrative activities, friendliness, activity and initiative, and leadership. The fact that no factor of technical competence emerged is probably attributable to the absence of any appreciable number of such items from the list.

Those aspects of the Southern California group's findings (21) which suggest characteristics of and techniques used by better supervisors are nontechnically summarized by Piffner (93). They include the following: good counseling techniques, not hypercritical of subordinates, confidence in higher management, good relations and influence with superiors, effective communication to employees, enforcement of discipline, not preoccupied with job security, has adequate authority, formalizes methods and procedures, decisive, uses good judgment, consistent and fair, does planning, and not particularly averse to paper work. These characteristics are based on perceptions reported either by supervisors themselves or by their subordinates. Although Piffner does not mention this, those familiar with the technical report will recall that the foregoing characteristics were not valid against all criteria nor in all samples in which they were measured. However, the list can serve as a helpful guide for future work on supervision, including selection, evaluation, training, and research on effectiveness.

For nearly a decade, a significant series of studies on administrative behavior has been under way at Ohio State under the leadership of Shartle & Stogdill. Some of their work has already appeared in journals; now, a series of monographs is being issued summarizing the program to date. The first volume (113) describes the major descriptive methods used, including sun-

dry interviews, check-lists, and ratings. The validity data are still rather scanty but are encouraging; reliabilities are somewhat on the low side but seem to be subject to improvement. In general the techniques constitute helpful suggestions of approaches that can be used in research on this complex subject. However, the reviewer has the impression they may not be intensive enough for more than exploratory purposes.

The second monograph (114) in the series describes some of the results obtained. Several of the techniques mentioned in the previous monograph were applied to a sample of naval officers. In general, variance in performance is more closely related to the type of position than to the type of naval organization in which the administrator works.

The relatively slight effects of interorganizational differences are further emphasized by another study reported in the monograph, showing that differences in patterns of administrative behavior between naval and industrial organizations are generally no greater than differences among either naval or industrial organizations. However, significant differences do occur among organizations within a group in certain of the behavioral characteristics, such as degrees of responsibility, authority, and delegation. It is encouraging for research in this field to know that there is a considerable degree of stability among organizations in behavior patterns for a given type of executive position.

A transposed factor analysis of 120 groupings of naval officers, using 46 behavioral variables, is described both in this second monograph and in a journal article [Shartle *et al.* (115)]. The factors identified are descriptive of types of positions: public relations representatives, professional consultants, personnel administrators, technical supervisors, schedule and procedure makers, maintenance administrators, directors or decision-makers, and coordinators. There are no intraorganizational factors, again indicating the relative unimportance of this source of variance. The findings suggest that selection and training procedures could be more effective if adequate cognizance were taken of the existence of behavioral differences among types of executive positions, and that different measures may be necessary in establishing criteria and in rating effectiveness. Duplication of this type of study, using industrial executives, would be most worth-while.

Brooks (11) also developed a list of managerial duties. Each of 96 executives in a company filled out how often he performed each function; immediate superiors of these executives also filled out the report on the 96 men, as did their subordinates. Leadership effectiveness evaluations of the 96 executives were made by both their superiors and subordinates. Significant differences were found in the activity descriptions of higher-level and lower-level executives, with the former spending more time on innovation, personnel relations, and coordination, whereas the latter put in more time on getting out production. Correlating behavior descriptions with effectiveness ratings accomplished by both superiors and subordinates showed that the executive is pulled between two sets of expectations that are not altogether congruent. Superiors tend to be impressed more with activities relating to administra-

tion, whereas subordinates give greater weight to skills of personal leadership. Brooks reports some trend (of unstated statistical significance) for executives to be rated relatively low by both superiors and subordinates if the executives' behavior is directed overwhelmingly toward the expectations of one or the other group. This double standard is not unrelated to the aforementioned dilemma which Argyris (3) poses regarding the directive and permissive roles of the executive; his hypothesis of the desirability of acting both roles seems to receive some support from Brooks' study. The feasibility of both roles is supported by the finding that the two dimensions of supervisory behavior, consideration and initiation of structure, are found to be only slightly correlated [Fleishman, Harris & Burt (28)].

TRAINING AND DEVELOPMENT

Psychologists are increasingly turning their attention to this topic, particularly in its application to managerial personnel. Symptomatic of this trend is the publication of a book on industrial training written by psychologists [Fryer, Feinberg & Zalkind (30)]; it places due emphasis on the importance of motivation and principles of learning in the conduct of training programs.

Nonsupervisory training.—In a radio code training program, trainees who progress more rapidly are found to be superior performers at the time they complete the program [Gordon (34)]. This measure of rate of progress would seem to have value either as a predictor of success or as a criterion.

Pilots are traditionally trained first on contact flying and then on instrument flying. An experiment by Ritchie & Michael (96) seriously questions the assumed validity of this procedure by showing that there is a 25 per cent reduction in the amount of practice required to attain standards including both contact and instrument conditions when the sequence is the reverse of the usual. However, with a total of only 22 subjects, this difference is not statistically significant. The study certainly deserves repetition with a larger sample. It is a good example of the benefits of experimentally evaluating different training methods.

Supervisory training.—Beliefs like that expressed by Haire (39) may underlie much of the current interest in this subject, viz., "... it does not seem at all out of line to hope for something on the order of eight or ten times as much improvement [in supervisory performance] from leadership and training as from selection." The assumptions underlying this hypothesis are not undebatable; for example, Haire expects good leadership and training to produce improvements of up to 50 per cent; yet, in estimating what may be expected from selection, he assumes that current best supervisors are only 50 per cent better than the worst. The hypothesis also appears to be excessively optimistic in the light of training results achieved to date. However, research is leading us to a better understanding of the conditions under which training does or does not result in some improvement.

Several reports have already been published concerning a comprehensive evaluation by an Ohio State group of a supervisory training program at

International Harvester. The entire study has now been reported in a monograph by Fleishman, Harris & Burt (28). One of the most important of its findings is that the effects of supervisory training may become neutralized when the trainees' superiors possess attitudes contrary to those developed through the program, pointing to the importance both of organization-wide training and of evaluating training results in terms of post-training on-the-job performance.

A commendable effort was made by Blansfield (7) to cope with the difficult problem of evaluating a "trainee-centered and non-directive" training program in which 46 managers had participated. The evaluative measures included post-training interviews with trainees, changes in behavior during the program as noted by the trainer and observer, trainees' self-ratings before and after coverage of the topics in the program, and changes in scores on Adorno's F-scale. Since no adequate statistical treatment was reported, the study is mainly of interest in its illustration of the variety of yardsticks that may be used for evaluative purposes. Researchers should, however, guard against the possible contamination of findings based on interviews and observations conducted by those who are strongly identified with the program they are evaluating.

Two studies were reported of training programs having the objective of improving a limited facet of supervision. McGehee & Gardner (80) evaluate the outcomes of an intensive course on time-study given to foremen and find significant improvements in tested knowledge and in ratings of the handling of time-study problems on the job. No significant changes in attitude toward time-study are revealed by an attitude scale.

Mann & Sparling (77) describe how employee absenteeism was reduced as a consequence of both supervisory training covering absenteeism and providing supervisors with uniform data on absenteeism among their personnel. The training was conducted both through conferences and literature.

Executive development.—Industrial interest in developing executives is high. An article by Drucker (24) cites good reasons for expecting a continuing need for such programs. However, psychologists have yet to become notably active in the training picture at this level. Some psychological consultants do seem to be extensively using counseling as an approach to executive development, but there is practically nothing in print on the subject; particularly needed here is evaluative research.

A study by Spencer (109) suggests two important types of problems which handicap executives and toward which training or counseling could well be directed. His survey of the difficulties of 950 company presidents finds that many are related to lack of time, in part created by a failure to delegate sufficiently. Many problems also involve procrastination in coming to grips with issues possessing emotional facets.

University training courses for executives are now popular. Anshen (2) points out the need for evaluating the usefulness of these programs and suggests that psychological measurement approaches can supply such information.

Argyris (4) reported a program of conferences being conducted by the top management personnel of a company among themselves. The conferences were aimed both at executive development and at helping solve current problems of the firm. The participants were dissatisfied with the program, feeling that it was accomplishing neither objective adequately. An interview analysis of their personalities suggests that most of them are strongly inclined to be active, structure-initiating, and self-sufficient. Argyris infers that such characteristics are contrary to those required of people who are to develop one another through group interaction, and that this was the basic reason for the executives' dissatisfaction with the conferences. Although it is not conclusive, the study does raise a question about the adequacy of this approach to executive development.

Coaching and counseling.—Personnel development is frequently undertaken by means of postappraisal interviews, where the appraisals are based on some sort of performance rating or record of observation. Flanagan & Burns (26) report the development and installation of a critical-incident performance record on which foremen make daily notations of employee behavior. This record serves as a basis for semiannual discussions between the foremen and their subordinates and is believed by the authors to serve this purpose better than did the impressionistic merit rating procedure previously used by their client. They emphasize that their performance record is neither a yardstick nor a rating method.

Employees in one company are better satisfied with a merit appraisal program when postappraisal interviews are conducted monthly rather than semiannually, according to a study by Zander & Gyr (128). The favorability of attitudes toward the system is related to employees' judgments of how well their supervisors conduct postappraisal interviews, indicating the importance of developing this skill in supervisors.

Hayden (46) draws a parallel between coaching and usual practice in postappraisal interviewing, i.e., both involve giving advice on the basis of a diagnosis. This approach may be appropriate, he indicates, when the need is to convey facts or impart skills, and when the problem is work-centered rather than person-centered. However, when the ratee's feelings or attitudes toward himself are concerned, nondirective counseling is more appropriate than coaching. He suggests that such counseling cannot be done within the traditional framework of postappraisal interviewing. These remarks point up the need for more research on methods and results in the coaching and counseling approaches to personnel development.

SELECTION

General.—What Meehl (83) has termed the "clinical" approach to prediction, i.e., one based on nonstatistical synthesis of data on an individual, appears to have been gaining prominence in industry in connection with assessment of executive and sales personnel. However, it has received little attention in print; especially noteworthy has been the absence of industrial

validation studies. Hence, two publications which appeared during the past year on this subject are duly welcome.

Hilton *et al.* (61) report a follow-up validation study of 100 individuals assessed for industrial clients by the clinical approach. The assessment data, which included mainly a battery of standardized paper-and-pencil tests, interviews by two psychologists, and, in some instances, projective tests, were reviewed by two psychologists. Their ratings, made on five scales, were then compared with ratings on the same characteristics made by company superiors of the appraised individuals. The correlation between these predictor and criterion ratings is .37, which represents an estimate of the over-all validity of the assessment procedure. A factor analysis of all 10 rating measures yields a general factor (over-all validity) plus two group factors corresponding to predictor and criterion ratings (halos).

The study design afforded the possibility that the criterion ratings were contaminated by knowledge of the reports, in spite of attempts to minimize this. Conversely, however, the heterogeneity of the sample with respect to jobs, companies, and criterion raters, plus the low reliabilities of some of the rating scales, undoubtedly operated to attenuate validity. In all, this was a commendable effort at validation on a retrospective basis; more such studies are needed. In the meantime it is reassuring to have evidence that the clinical assessment procedure used by one industrial consulting agency appears to be usefully valid.

Under the term "analytic" method, Stern, Stein & Bloom (112) set forth in their book one of the most complete expositions of a clinical approach to assessment. Their discussion is applicable to industry, although their orientation and illustrations refer mainly to student personnel. Their method places a commendable emphasis on the careful development of job specifications. Unfortunately, these are deliberately based solely on the expectations and perceptions of the "significant others" under and with whom the candidate is to work. Important as these perceptions may be, both the candidate and the organization have reason to expect the psychologist to predict competence in terms of standards more universal than just the local stereotype. The authors also espouse greater reliance on projective techniques rather than objective tests (the reverse of what this reviewer feels is justified by evidence), and the praiseworthy use of an intensive case conference for synthesizing assessment data. Their otherwise scholarly treatment is marred by the failure to include other than scanty data on the validity of these procedures.

Stern, Stein & Bloom also outline the "empirical" or statistical approach to assessment. Their third or "synthetic" approach refers essentially to a method for developing a global measuring device, rather than to a complete approach to assessment. Their "configurational" approach, which is a subspecies of the statistical one, grows out of the hypothesis that a given level of criterion performance may be attained through any one of several configurations of traits, thus not being capable of maximally accurate prediction

by means of a single multiple regression equation. Among their suggestions is the identification of subsamples by transposed factor analysis and the development of separate equations for each. An interesting verification of the potentialities of this approach for improving prediction is cited. This matter of configurational subpopulations is well worth careful attention in selection research.

The time-honored actuarial or statistical approach continues, happily, to dominate the psychological literature on industrial personnel selection. A number of such studies is summarized below. In turning to them, mention should be made of a new book by Stone & Kendall (116) which emphasizes this approach; it is a competent treatment of the procedures of recruiting, selecting, and inducting employees.

Methods other than tests.—Most of the selection studies reported relied mainly or entirely on tests. First, however, several studies based on other assessment techniques will be considered.

Yonge (127) investigated the validity of a 15 to 20 min. interview procedure which eventuated in a rating by the interviewer on six scales, plus a general evaluation. The latter was found to correlate between .45 and .99 with composite supervisors' ratings in five small groups of pharmaceutical employees. Three of the five coefficients were statistically significant.

The value of biographical data for predicting turnover has again been demonstrated, once with various types of airline personnel [Krantz (66)] and once with seasonally employed production workers [Dunnette & Maetzold (25)]. The predictive value of the data is found to persist on cross-validation in the latter study, both over successive seasons and in other plants. Collins (15) reported that height is significantly correlated with production records and with supervisors' ratings in a small sample of metal polishers.

Ricciuti (95) finds that aptitude-for-service ratings made on naval cadets correlate about .25 (statistically significant) with ratings of post-graduation shipboard performance. Aptitude tests and academic grade were practically uncorrelated with the shipboard ratings. The utility of ratings for prediction purposes is hereby once more demonstrated.

Nonsupervisory personnel.—Knauff (65) reported a follow-up validity study of a type that should be more often done. A short mental ability test is found to correlate .6 with the job level attained by office employees after from 5 to 17 years of employment, showing that the test is a good predictor of promotability (or else that it was heavily weighed in determining who should be promoted). The test also predicts production records, with validity coefficients ranging from about .3 to .5 in four office departments.

In another clerical study, a test consisting of substitution and checking is found to correlate appreciably with training grades and performance ratings received by card-punch operators [McNamara & Hughes (82)].

Several validation studies were reported for military occupations. The final grades received in an aircraft and engine training course are found to be predictable on the basis of a problem-solving test [Gaier (31)]. Siegel & Jensen (103) developed a performance test of proficiency in troubleshooting elec-

trical equipment, and they report that it discriminates moderately well among naval electricians at different classification levels. A battery of auditory perception tests was found to be valid for measuring success in radio-operator training [Fleishman (27)]. A study reported by Allison (1) suggests that different prediction equations may be needed for candidates with different backgrounds of experience.

Two studies appeared on automobile drivers. Venables (120) presented evidence that inconsistency in acceleration and deceleration is associated with questionnaire measurements of neuroticism and extreme introversion or extraversion. Lauer (69) reported a multiple correlation of .38 between ratings of Army drivers and a battery of paper-and-pencil tests; this is higher than the validity found for batteries of psychophysical tests of the type traditionally used in driver selection.

Harrison, Hunt & Jackson (41, 42) assessed the abilities and interests of 240 mechanical engineers by means of tests, biographical data, and an interview. The personalities of 100 of these engineers were studied both by tests and an interview [Harrison, Tomblen & Jackson (43)]. General descriptions are offered, and normative data and intercorrelations are reported for some of the tests. No data regarding validity are furnished.

Comrey & High (16) administered the Kuder Preference Record and the California Test of Mental Maturity to supervisors in 29 production departments of an aircraft company. Mean scores on each of the tests were computed for the supervisors in each department. These means were not found to correlate significantly with objective records of departmental efficiency and quality. Note the unusual use of the department as the unit of analysis.

Rainio (94) tested groups of foremen in Finnish factories with a wide variety of instruments, including situational, objective, and projective tests. Criteria included superiors' ratings, sociometric measures, and departmental output and labor costs. Among the measures showing significant validity are situational (group task) tests, arithmetic, and the Body Sway test of suggestibility (negative). Situational tests again show up as having more value for managerial selection than their limited use in this country would indicate.

Ghiselli (32) has developed an "initiative" key for his forced-choice self-description inventory. Initiative scores were found to correlate .24 with job performance ratings of foreman, .35 with ratings of managers, and -.29 with ratings of nonsupervisory personnel. Also, managers are found to have higher mean scores than nonmanagers. The author neglected to include tests of statistical significance; from the data provided, the reviewer estimates that the above results are significant at the 5 per cent level, or nearly so.

The Tomkins-Horn Picture Arrangement Test is reported to reveal the following significant differences between executives and nonexecutives: executives more frequently show fear of illness (interpreted as indicating fear of failure) and react to problems with a feeling of dependence on others for a solution; the latter is interpreted as reflecting executives' ability to get the co-operation of others in solving problems [Miner & Culver (86)]. Although

the authors purport to illuminate personality characteristics which "make for success in the business world," they do not seem to feel obliged to correlate their variables against differences in the performance of executives; this would, of course, be necessary for the attainment of their stated objective.

Psychologists are publishing relatively little research on the problem of identifying executive potential. Such research is particularly needed in view of the considerable amount of service along these lines which they seem to be providing to industry.

PERFORMANCE RATING

The two preceding sections have illustrated how performance ratings are used in personnel development, for prediction of job success, and as criteria. The present section is devoted to research on rating methodology. In general, there were no revolutionary developments, but some useful information was added.

Stogdill & Shartle (113) describe a procedure for obtaining effectiveness ratings of administrators. Evaluators both rank the ratees, and rate them on a four-point scale. A scoring procedure has been developed for combining both the ranking and rating received by an individual into a single score. The system results in normal distributions of scores. A conservative method of estimating inter-rater reliability yielded a coefficient of .46.

In a rating scale in which the rater checks the statement of degree which best describes the ratee, scores based on arbitrary weights and on standard deviation scaled weights have a correlation near unity [Jurgensen (62)]. There are no differences between the two types of scoring systems with respect to split-half, alternate-form, or inter-rater reliability.

Two studies were reported which attempt to analyze what it is that raters rate. Grant (35) performed a factor analysis of superiors' ratings of division managers on 20 scales. The analysis yielded a general factor ("halo") plus five group factors, suggesting that the proper way of scoring the form would be in terms of six scores corresponding to these factors. There seems to be little specific variance, beyond error, in any of the scales.

Hausman & Strupp (44) obtained supervisors' ratings on 55 aspects of the performance of aircraft mechanics. An intercorrelation analysis of these scores suggests a dimension of technical competence plus several nontechnical dimensions; the dimensions are not uncorrelated. Scores on the technical dimension are found to correlate more highly with a test of technical proficiency than is true of the nontechnical dimensions, thus supporting the distinction between these two aspects of rated performance.

There were several investigations of conditions that may be hypothesized as affecting ratings. In a study of merit ratings on and by executives, no evidence is found on bias growing out of the relative ages of the rater and ratee [Maher (75)]. Mandell (76) reports that supervisors who receive relatively low merit ratings are poorer raters, in the sense that the ratings they make have lower correlations with the consensus of other raters. Also,

"tough" raters are usually younger and have shorter service in grade than lenient raters.

Lovell & Haner (73) developed a forced-choice form whereby students could rate college instructors. Both test-retest and split-half reliability approximate .9. When students are asked to bias ratings deliberately, the resulting average scores are only slightly beyond chance scores. This leads the authors to conclude that the forced-choice rating is less prone to deliberate prejudice than is true, say, of graphic ratings.

CONDITIONS OF EFFICIENT WORK

This general subject loomed large in the purview of the applied psychologists of a generation ago. It now has dwindled to less than one-tenth of the total published output in industrial psychology.

Equipment design.—This relative newcomer is now the most active phase of the subject. There is lively interest in dial reading. For example, in a study of dial symbols, wider strokes are reported to be optimal in low illuminations; the visibilities of white symbols on black backgrounds and the reverse are not differentially affected by illumination level [Soar (108)]. Dial reading is more effective under brighter illuminations, but it is not affected by the brightness of the preceding visual task [Spragg & Wulfeck (110)]. Readings of peripherally located dials may be made with considerable accuracy, up to an angle of about 40 degrees [Senders, Webb & Baker (102)]. After practice, check-reading is about equally effective for sets of dials either uniformly or symmetrically aligned, although initially the latter alignment is more difficult [Ross, Katchmar & Bell (99)]. In scale settings, markings several millimeters in thickness result in fewer errors than hairline markings [Topmiller (117)].

Two well-designed studies appeared on tracking-display systems [Chernikoff, Birmingham & Taylor (13); Pearl, Simon & Smith (91)]. Space does not permit adequate summarization of their features here, but their reading is recommended to those interested in the subject.

Research on control devices includes a study by Green & Anderson (37) showing that shape-coded handles, of a type usable for electronic switch panels, are valuable in reducing confusion and errors; this confirms earlier research on the shape-coding of aircraft controls. Bugelski (12) demonstrates that untrained individuals are likely to give erroneous pedal reactions to off-center indicators (such as the "ball-bank" indicator) in which the proper reaction involves use of the foot on the same side as the indicator displacement. Ross (98) contributed a study showing the feasibility of determining subjects' preferences for certain relationships between visual displays and control movements; this technique would have considerable practical value if it were found to be predictive of performance.

While recognizing the hazards in making value judgments of research topics, this reviewer nevertheless feels obliged to report his experience of *déjà vu* on reading much of what is being done on equipment design. Topics

like dial reading have been intensively cultivated whereas broad fields, such as the operation of semiautomatic machinery, have scarcely been touched. Might this not be one reason for private industry's relative apathy?

Working conditions and methods.—Mayo (79) compares the training achievement of matched groups, one of which was trained in an air-conditioned building with a median temperature of 70 and the other in non-air-conditioned buildings with a median temperature of about 80. The differences in achievement, although slightly favoring air-conditioning, are not statistically significant.

A performance decrement is found in the course of seven hours work in a skilled task involving a complex visual field [Hauty & Payne (45)]. Contrary to previous evidence, the degree of decrement is not greater for performance based on more peripherally located visual instruments.

Smith & Lem (107) report greater efficiency in production when work lots are composed of fewer rather than more units. This is attributable to a significant positive relationship between the number of units composing the lot and both the number and frequency of voluntary rest pauses taken by the workers. The results may be interpreted as reflecting the effect of the need for task-completion. Incidentally, the authors made an awkward choice of terminology by calling these informal rest pauses "work stoppages," since the latter term is commonly used in industry to designate refusal to work, i.e., strikes.

MENTAL HEALTH

The problems of mental health and maladjustment are being brought ever more forcibly to the public's attention. It is therefore not unexpected that overtones are beginning to be heard on the industrial scene.

One of the literature surveys prepared by Herzberg *et al.* (56) deals with mental health in industry. Their report reviews the literature on interpersonal and emotional factors in workers' accidents, alcoholism, and absenteeism, and describes industrial mental health programs. The authors conclude that there are very few validated facts to support widespread opinion on these topics.

Menninger & Levinson (84) present a case for what they call "psychiatry in industry." They discuss the need for dealing with certain specific medical or psychosomatic implications of absenteeism, accidents, and alcoholism, plus the need for counseling of executives and others who may need it. There is little doubt that additional research and service along these lines would be well worth while. In addition, the authors propose attacking certain broad questions such as: What does a man want out of his work and how does he try to get it? How does work relate to status and prestige and how can it be made to enhance them? What are the "health-giving" contributions of the work group and how can they be fostered? What can the understanding of human motivation contribute to the alleviation of labor disputes? The authors do not seem to recognize fully that psychologists and other social scientists have been grappling with these broad issues for a couple of decades

and not unsuccessfully. It is true that the objectives have usually not been phrased explicitly in terms of mental health, but rather in terms of attitudes, satisfaction, and effectiveness. But these are probably sounder concepts, and certainly more operationally definable, than "mental health," a concept which often misses the point by being constituted as the opposite of mental aberration.

This is not to say that we can rest on our laurels. In addition to much more intensive work along present lines, there is also, for example, a particular need for investigating our independent variables more specifically against criteria of emotional stress. A study by Neel (88) sets a noteworthy example as a beginning. He administered a questionnaire containing items both regarding anxieties on the job and regarding attitudes and perceptions relating to various job aspects. The "anxiety" scores are found to correlate between .2 and .3 (significantly) with adverse attitudes toward foremen, working conditions, the nature of the job and work, company, wages, promotion policies, and time-study system. Of course, there must be caution in interpreting cause-and-effect relationships. However, the dependence of anxiety on job conditions and practices is suggested by the fact that employees who actually do work on time-studied jobs express more anxiety on the questionnaires than those who do not. The analysis of anxiety measures in relation to objective descriptions of conditions would, in general, avoid the ambiguity involved in a two-way comparison of attitudes. Nevertheless, the study is a commendable and suggestive attack on this important subject.

CONSUMER RESEARCH

Only two of the seven articles selected as relevant to this topic appeared in psychology journals, and only two were written by American Psychological Association members. Psychologists working in this field either are not doing work which they feel is appropriate for technical publication, or they are not publishing reports of such research. Probably both reasons are operative to a degree.

One of the most clearly psychological articles is that by Heller (47), testing the Gestalt principle of closure in relation to the memory value of advertising slogans. He reports that more slogans are remembered in which every seventh letter is omitted than is true of complete slogans.

The topic of "motivation research" continues to receive lively attention from popular and trade publications. Although much of the interest and controversy center around psychological issues, especially the use of projective techniques, no relevant technical or research article by a psychologist appeared during the year. There were three articles in nonpsychological technical journals, all written by nonmembers of the American Psychological Association. Rothwell (100) criticizes the use of projective techniques essentially on the grounds of their questionable validity and feels that their use in motivation research goes beyond the conditions under which they may be helpful to the clinical psychologist. She ignores the limited number of studies which have indicated their value in predicting consumer behavior and which

have shown their amenability to door-step use, but, in general, her criticisms are not irrelevant. Bogart (9) examines these and other criticisms, concedes that they may have some foundation, and rebuts most of them on logical and assumptive grounds. He concludes that they have a place in complementing more conventional quantitative methods, but no data are presented.

An article by Zober (129) describes how to devise pictorial tests resembling the Thematic Apperception Test and Picture-Frustration technique, and how to use them in marketing research. No data are presented regarding the reliability or validity of the instruments he used. A saving grace is that Zober proposes that the techniques be used only as sources of hypotheses to be employed in interpreting data collected by objective questionnaires; however, it would seem possible to determine whether such interpretations are more predictive of consumer behavior than are those made in the absence of the projective data.

Apropos of this entire subject, one of Rothwell's proposals is indisputable: that more research needs to be done in which measures of consumer attitude and motivation are systematically correlated against consumer behavior. In this context, the following studies strike a refreshing note.

Benson (6) has developed a mathematical model for predicting consumer behavior, based on the theory that the consumer seeks to spend his money in such a way as to obtain commodities of maximum preference to him. Application of the model requires knowledge both of prices and of consumer preferences for commodities. In an exploratory test of the model, it was possible to predict the average prices of foods purchased in a restaurant within a mean error of six cents.

Using a consumer panel, Cunningham (23) demonstrated that there is validity in the concept of brand loyalty. Many instances were noted in which at least 90 per cent of a family's purchases have been concentrated on a single brand over a period of three years. Guest (38) finds that brand loyalty perseveres to a considerable degree; there is 35 per cent agreement between the present preferences of young adults and the preferences they expressed 12 years previously while still in school. The average degree of agreement between early preferences and present use is 27 per cent.

OVERVIEW

The literature of the past year has had its share of studies with pedestrian hypotheses, too few cases, poorly measured variables, and inadequate designs. However, there is much work of high quality being done, perhaps in higher proportion than ever before.

The trends promise even greater events for the future. Among them is the previously noted increase in the proportion of research which is integrated around consistent themes. Methodology also shows signs of improvement; there are continuing efforts to refine criteria, objective and behavioral measurements are gaining on subjective ones, and there is something of a trend toward experimental manipulation of variables in field situations. Research designs are taking better account of the multiplicity of variables affecting industrial behavior.

Although there are signs that more industrial psychologists are addressing themselves to issues of greater social, practical, and theoretical import, too much effort is still being dissipated in the pursuit either of minor refinements or of unexciting objectives. The discovery and investigation of significant variables and issues constitute possibly the most solemn obligation that the scientist has toward society, his profession, and himself. Such issues now germinating in industrial psychology appear to include: the ingredients and consequences of directive and permissive organizational climates; the identification and development of executive ability; the improvement of molar man-machine-methods systems; facilitating integration among financial, social and personal goals of employees and the alignment of these goals with those of the company; the objective measurement of individual and organizational performance effectiveness; and the interrelationships among management practices, employee characteristics, attitudes, and performance. This list is by no means exhaustive.

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ABNORMALITIES OF BEHAVIOR^{1,2}

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This review has made no attempt to cover non-English publications or to deal with all the literature which might possibly be relevant to what could reasonably be considered as abnormal behavior. Selective preference was given to quantitative clinical and experimental investigations, to areas showing research activity, to publications which might have hypothesis-making possibilities, and to topics which absorb the energies (teaching, research, and clinical) of substantial numbers of psychologists. A limited number of books have been singled out for brief comment without implication as to the relative merit of other publications (43, 103, 157, 192) which may only be noted bibliographically.

Jones (91) covers the period from 1901 to 1919, a period of high productivity both in theory building and in clinical contributions, in his second volume on Freud. With careful accuracy the biographical details are skillfully woven into an account of the personal man as well as the scientific man with his creative hypotheses about normal and abnormal human behavior.

The Psychology of Personal Constructs by Kelly (101) is an important two-volume contribution to the understanding of abnormal behavior, one which will need time for absorption and appreciation of its theoretical, clinical, and research implications by the clinical workers in the field. Wallen's introductory textbook to the clinical method in psychology (194) promises to be useful in helping the beginning student acquire some perspective as to the methods by which abnormalities of behavior can be studied in life situations. The casebook by Weinberg & Hire (198) includes very limited psycho-socio-medical diagnostic data and does not seem to point up the significant problems which the case selections were intended to illustrate. The Burton & Harris (24) casebook of clinical studies of personality is in sharp contrast to this book and provides a useful source book of clinical material. The report of the Michael Reese Hospital group (14) on their field investigation of anxiety and stress is an interesting account of the methodological problems which interdisciplinary study of a (biopscho-social) life situation (the training of paratroopers) presents, its limitations as well as its potentials.

INDUCED BEHAVIOR CHANGES (103)

Adrenochrome.—The adrenal glands, their secretions and the metabolic fate of the hormones, have long had an interest for investigations of emer-

¹ The review surveys the literature from April, 1955 through April, 1956.

² The following abbreviations are used in this chapter: EEG (electroencephalogram); LSD-25 (*d*-lysergic acid).

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gency behavior, emotionality, and anxiety potential. Osmond, Smythies, and the Saskatchewan group (137) call attention to a natural oxidation product of epinephrine (adrenaline), adrenochrome, which they believe to have schizophrenogenic properties as well as a capacity to induce EEG abnormalities. When injected into the blood stream of normal subjects, a "model psychosis" can be produced. Brain metabolism, as studied on the Warburg apparatus, was found to be markedly inhibited. Szatmari, Hoffer & Schneider (184) were not able to confirm the schizophrenogenic reactions in a small group of normal subjects. However, in 8 out of 21 epileptic subjects, they obtained reports of feelings of strangeness and unreality, sleepiness, feelings that a seizure was imminent, and EEG changes (decrease in basic frequency, 30 per cent decrease in voltage, an increase in dysrhythmic pattern and hypersynchrony). High intravenous dosages of niacin in the epileptics seemed to reverse the effect attributable to adrenochrome.

Lea (105), on the basis of potential biochemical relatedness of adrenochrome to melanin (a pigment substance) and the antihistaminic action of adrenochrome, hypothesized a higher incidence of dark eye-hair coloration and a lower incidence of allergy problems in schizophrenics. A highly significant excess of dark hair color was found for schizophrenics but only in the 15 to 19 year age, and one of 500 schizophrenics had an allergic condition as compared with 17 in the group of the 500 control subjects. Lea suggests a mechanism of hepatic insufficiency (either hereditary or environmental in origin) as a basis for the heightened adrenochrome levels which predispose to schizophrenic reactions.

Weil-Malherbe (197) notes the lowered blood-plasma epinephrine level which is associated with decreased mental activity in various forms of mental deficiency, in senile dementia, and under certain drug conditions (i.e., barbiturates).

d-Lysergic acid (LSD-25; *d*-lysergic acid diethylamide).—There is little doubt that LSD-25 produces profound transient disturbances of behavior in human subjects. Cautious investigators describe the reaction as a "model psychosis" (48, 50, 167), but the Boston group (Harvard Medical School, Boston Psychopathic Hospital, Worcester Foundation) (152) consider the response to LSD-25 more adequately categorized as a moderately acute schizophrenic upheaval with or without catatonic features.

Rinkel *et al.* (152) noted that under LSD-25 intoxication a much higher proportion (26 per cent) of 100 normal subjects exhibited distortions of hostile or affiliative relationships than showed distortions of impersonal, empathic, and nutrient kinds of relationships (6 per cent). Psychological tests revealed lowered organization and integration of response to the conventional features of the environment, centering of attention on minute or discrete aspects of experience, lessened intellectual and emotional control, and a lack of discrimination between own feelings and those of others during the toxic state.

Savage (162) believes that the hallucinatory experiences, the distorted time-space relationships, the heightened experience of anxiety, the deper-

sonalization, and the estrangement feelings produced by LSD-25 are a result of an intensification and extension of ego body feeling. Distortion of body ego feelings leads to a loss of ego boundaries with a subsequent confusion between the real and the unreal.

The capacity to produce an experimental model psychosis leads quite naturally to the search for some means of modifying or blocking the appearance of this disturbed response. Agnew & Hoffer (2) gave 200 mg. of nicotinic acid (intravenously) to five normal subjects at the peak of the LSD-25 reaction. This led to a marked reduction of all LSD-induced disturbances except in the affective area. Pre-LSD intake of nicotinic acid modified the LSD experience to some extent in the area of concentration and vision but did not alter affective disturbances, unco-operativeness, disturbances in self-identity, and de-realization to any significant extent. Schwarz, Bickford & Rome (167) gave 25 mg. of chlorpromazine intramuscularly to 13 physician-subjects 2 hr. after ingestion of the LSD-25 and obtained a reversal of the induced psychosis at the presumed peak of the LSD reaction. Fabing (48) and Fabing & Hawkins (50) have demonstrated that LSD-25 induced psychosis could be terminated very rapidly by intravenous injection of Frenquel [alpha-(4-piperidyl) benzhydrol hydrochloride] or could be almost completely blocked by oral administration of Frenquel for several days prior to the LSD-25 intake. Visceral effects associated with LSD-25 experience were still noted, even though the psychic effects were essentially controlled by the use of Frenquel.

Rinaldi & Himwich (150) produced a persistent EEG alertness pattern (fast, low-voltage cortical activity with a 4 to 6 cycle/sec. thalamic rhythm) with LSD-25 in the rabbit. The administration of Frenquel restored the normal resting EEG pattern. Frenquel alone produced no EEG changes. Schwarz and his colleagues (167) noted a similar restoration to normal from the LSD-25 disturbed EEG pattern in human subjects following chlorpromazine injection.

A notable attenuation of LSD induced symptomatology in neurotic and psychotic patients by the use of reserpine and chlorpromazine is reported by Giberti & Gregoretti (67). Chlorpromazine seemed to modify the mood and affect more significantly, whereas the reserpine had a greater attenuating effect upon psychomotor activities, including the anorexia and nausea frequently associated with LSD psychosis. The chlorpromazine and reserpine, given either prior to or during the course of LSD induced psychosis, are effective blocking agents.

Chronic, regressed schizophrenics reacted to LSD-25 by symptom intensification, symptom reversal, and the appearance of unusual behavior. Cholden, Kurland, & Savage (31) found that repeated administration of the LSD-25 led to rapid drug tolerance which could not be overcome with increased dosage, in contrast with normal subjects who do not seem to develop such a tolerance. A five day lapse, however, seemed to restore reactivity to LSD-25 in the schizophrenic subjects.

Frederking (57) utilized the capacity of the individual to recall his cogni-

tive and emotional experiences under LSD-25 and mescaline intoxication as a technique for accelerating psychotherapy or overcoming blocking in the treatment process. The intense hallucinatory state, the symbolisms, and the affective reactions could be utilized in the therapeutic relation for their psychocathartic effects.

Hallucinogens and schizophrenia.—The demonstration of the presence of serotonin (5-hydroxytryptamine) (22) in brain substance may provide a link for the metabolic theories which seek to relate the hallucinogenic behavior reaction to the biochemistry of schizophrenia (i.e., schizophrenia as associated with a deficiency of serotonin) (114). Shore, Silver & Brodie (172) have shown that serotonin and reserpine exert a common central nervous system potentiating action that is antagonized by LSD-25. Pletscher and co-workers (143) found that reserpine can directly effect the release of serotonin from body depots where it is stored (e.g., the intestine). Presumably the quieting action of reserpine is associated with the release of serotonin from brain tissue (22). Fabing (49) has extended the hypotheses of metabolic error as a significant etiologic factor in schizophrenia by stressing the role of the indole nucleus in many drugs which have been associated with the production of model psychoses [e.g., epinephrine-adrenochrome, mescaline, LSD-25, bufotenine (*n*-dimethyl-serotonin), etc.].

Tyler (190) noted the occurrence of psychotic-like reactions in all adult subjects after prolonged sleep deprivation. The reactions ranged in severity from mild feelings of depersonalization to extreme states resembling acute paranoid schizophrenia. Symptoms disappeared after sleep. The parallel between the sleep deprivation reaction and toxic psychoses seemed very marked.

ORGANIC BRAIN STATES

MENTAL SUBNORMALITY (23a)

There are many indications that the long period of relative research inactivity in this field [e.g., a review of clinical psychology without a single reference (23), and less than 10 references in the total annual output of five psychological journals concerned with clinical problems] is coming to an end. Meyer (122) points out that this situation is apt to change very significantly within the next few years in response to the pressures by the parents of mentally subnormal children for more intensive research into the etiology and prevention of mental deficiency, and for the development of new educational and psychological methods to extend the intellectual, emotional, and social spheres of the mentally subnormal children. The trend away from a "custodial" concept to a "treatment" concept in institutions for mentally subnormal children could be a vital factor in facilitating the development of research programs.

Diagnosis, etiology, and psychopathology.—The World Health Organization Committee on the Mentally Subnormal Child (17) has proposed a terminology based on degree of subnormality. Three degrees have been suggested: mild subnormality—IQ 50 to 69, Adult M.A. 8 to 12; moderate sub-

normality—IQ 20 to 49, Adult M.A. 3 to 7; and severe subnormality—IQ 0 to 19, Adult M.A. 0 to 2. The term "mental retardation" is to be reserved for those children whose educational and social performance is markedly below that which could be expected on the basis of their known intellectual abilities.

Benda & Farrell (17), in the summary of their monumental work on the psychopathology and neuropathology of mental deficiency (which included the gross and microscopic autopsy studied of 267 individuals), emphasize that research on mental deficiency must seek to delineate the different groups of mentally subnormal individuals. It is their conclusion that severe and moderate forms of mental subnormality are attributable to factors which operate either in the prenatal, neonatal, or postnatal periods. Genetic factors have been grossly overestimated as a cause of severe forms of mental subnormality. Congenital acromicria (mongolism) is the most frequent congenital malformation which occurs in the prenatal period; neonatal disorders are primarily associated with factors which interfere with the oxygenation of the brain; and traumatic and infection factors are the most important contributing factors to mental subnormality arising in the postnatal period. The majority of mental subnormalities are of the milder form, and it is these which are most probably associated with as yet poorly defined genetic factors.

McQuarrie (118) noted that 25 out of 40 infants in whom hypoglycemic states were revealed in the course of pediatric study showed marked retardation of development associated with the extent and duration of their deviant glucose metabolism. Treatment with adrenocorticotrophic hormone until spontaneous adjustment took place or until the child's homeostatic mechanisms developed prevented permanent brain damage. Those children in whom the hypoglycemia had gone undiagnosed, under the impression of idiopathic epilepsy, showed permanent damage, with the destroyed areas of the brain acting as irritative lesions to maintain convulsive reactions. Hartman *et al.* (79) reported another type of disturbed sugar metabolism (galactosemia) which also seemed to result in a toxic reaction with marked growth failure, mental subnormality, bilateral cataracts, and hepatomegaly. Under a special dietary regime which excluded galactose from the diet, these children tended to showed marked improvement mentally and physically.

Perhaps the most widely known metabolic disorder associated with mental subnormality is that of phenylpyruvic oligophrenia. Woolf, Griffiths & Moncrieff (207) report a successful dietary regime in which phenylalanine was low to almost absent. Three children (two idiots, one imbecile) were kept on it from 4.5 to 10 months with a significant increase in mental age, a rise in IQ, a decrease in convulsive seizures, and the possibility that these children might reach an educable level. The importance of early diagnosis and dietary therapy for these biologic deficiencies is obvious even on the basis of the limited results available.

Nolting (130), proceeding on the hypothesis that vitamin C is significantly associated with cell growth especially in the developing fetus, made

an extensive statistical analysis of the birth records of 7000 mentally subnormal children in England. He reported that the birthlines of these children differed significantly from those of the normal children in that they showed a peak incidence during those months immediately following the months with the greatest vitamin-C shortage. From this he infers that a disposition for debility may be developed during the last three months of gravidity because of vitamin-C deficit.

O'Gorman (133) takes issue with the belief that mentally subnormal individuals are more prone to psychotic upset than persons of normal intelligence. The alternative proposition must be carefully examined, namely, that the mental defect may be a result of a psychosis in youth. An intensive study of a group of adult female moderate-to-severe subnormal women led him to conclude that 29 per cent of them were psychotic, with the clinical picture in each case indistinguishable from that of deteriorated schizophrenia. Improved differential diagnosis, especially at the younger age levels, might lead to more definitive treatment.

McCulloch, Reswick & Roy (116) explored some of the differences in word learning ability in two groups of mentally subnormal individuals who were matched for mental age but had a wide difference in mean chronological ages (16 versus 48 years). A single trial and a repetitive trial learning procedure were used. The young group excelled the old group in gain, both groups were equal in grasp, and the total scores did not reliably differentiate between the two groups. In a second study McCulloch and his associates (115) demonstrated that low positive correlations existed between certain more global measures of intelligence (e.g., Stanford-Binet, Form M) and "grasp" and "gain." The achievement on tests of general intelligence thus may serve as an important index to certain learning capacities, but the authors conclude that there may be other types of learning (i.e., those associated with much repetition) which are not very adequately predicted on the basis of such tests, yet which occur very frequently in the life of institutionalized mentally subnormal children.

Cantor & Hottel (28) report the amazing finding that mentally subnormal adolescents at the upper IQ levels consistently perform better in a discrimination task than the lower level IQ groups regardless of whether they are given one peanut or four peanuts for a successful trial—*sic transit gloria!*

Four groups of male imbeciles were trained on a simple repetitive manual task under four different conditions of motivation by Gordon, O'Connor & Tizard (73). Goals based on previous performance (self-competition) seemed to lead to the best performance levels. A change from group competition or control condition to self-competition also resulted in increased performance levels. The meaningfulness of self-orientation for motivation at this level of social-emotional development may be of importance for educational procedures with the moderately to severely subnormal individual.

The hyperactive, assaultive, destructive, mentally subnormal child in an institutional setting presents a problem to himself as well as to the general

training program of the institution. Bair & Herold (11), Rettig (146), and Bonafede (21), each working independently, came to similar conclusions about the use of chlorpromazine as a calming agent. Bair & Herold placed 10 of the most hyperactive children (matched with a control group) on a chlorpromazine regime. Not only was there a significant decrease in the pattern of hyperactivity, but other positive behavioral gains were noted. A statistically significant gain in IQ was made by the experimental group over their own pre-chlorpromazine level as well as over the matched group. Rettig's group consisted of 27 mentally subnormal individuals who had failed to benefit from barbiturates, electroshock therapy, occupational therapy, and psychotherapy. With chlorpromazine medication, 23 showed from fair to excellent improvement, and the remaining four, who showed poor or no improvement, were characterized as having severe brain damage. Chlorpromazine has made such a significant contribution to the problem of management of the disturbed mentally subnormal patients that it has released institutional personnel from the purely custodial to the more direct treatment activities and enhanced the therapy resources without increase of budgets. Bonafede found chlorpromazine to be highly effective and dramatic in controlling behavior disturbances in mentally subnormal epileptic women. The drug had to be used along with anticonvulsive medication, for, by itself, chlorpromazine has shown no appreciable effect in reducing the frequency of seizures.

Two sharply contrasting experiences with group therapy with institutionalized mentally subnormal adolescents are reported by Vail (193) and Ringelheim & Polatssek (151). Vail's group was composed of aggressive, disturbed patients with a preponderance of organic brain pathology and with a range of IQ from 35 to 70. There was a fixed type of nondirective therapy, no control over group membership, and no preparation of the institutional setting. Follow-up results a year after termination of therapy showed that those individuals who had attended the fewest number of sessions had shown the greatest amount of improvement. Ringelheim & Polatssek, on the other hand, made careful preparation of the institutional setting so that employees who were involved in the everyday life of these patients were included in the planning of time, place, and purpose of the therapy sessions. Group membership was carefully selected for the initial groups; a co-therapist was used, and the therapeutic approach was kept flexible in order to cope with the individual levels represented in the group. Typical problems of establishing criteria for evaluation of change were encountered, but the authors feel a positive therapeutic contribution was made.

Astrachan (9) reported similarly favorable results with groups of female adolescent and adult mentally subnormal groups. Her conclusion is that discussion group therapy in an institution for the mentally subnormal is a treatment resource for those mentally subnormal individuals diagnosed as familial or undifferentiated, especially those who present problems associated with depression, passivity, and mild paranoid trends.

EPILEPSY AND THE CONVULSIVE DISORDERS

Etiology and psychopathology.—Kallmann & Baroff (92), in their review of genetic factors in the convulsive disorders, emphasize the general unsatisfactory nature of current statistical data in supporting heredity as a significant factor in the etiology of epilepsy. Yet persistent research interest in this hypothesis remains, with extension of the notion to include a general inherited organic brain disorder that relates epilepsy, schizophrenia, the primary behavior disorders, and psychopathy. Schacter (163) studied these four disorders in one family by means of the case study method, EEG, and psychological tests. The relatively unstable electrocortical patterns demonstrated in several members of the family were taken to be an expression of an underlying brain disturbance which constituted a predisposition towards their abnormal psychological reactions to stress. Thompson (187) noted an association of some cases of sexual psychopathy with psychomotor epilepsy—where extremely bizarre psychopathic behavior can occur with no recollection by the individual. Pathological alcoholic intoxications, in his judgment, are just another type of psychomotor attack that differs in that it requires a certain alcohol concentration in the blood stream to be precipitated. Correlative EEG evidence is presented to show that the same fundamental cerebral disturbance may underlie these three behavior reactions.

Ervin, Epstein & King (47) found that of 42 patients who showed unilateral or bilateral temporal spike foci in their EEG's, 34 carried a diagnosis of schizophrenia, and 31 of these 34 had clinical epilepsy, usually psychomotor epilepsy. The patients with the most severe psychiatric disorder were those with psychomotor seizures.

Denber's (42) investigation of the effects of mescaline on the clinical and EEG reactions of 12 patients with grand mal and petit mal epilepsy led him to a conclusion directly opposite to that of Schacter. He felt that some basic differences exist in the patterns of cerebral organization of epileptics and schizophrenics because of the absence of psychotic-like reactions to mescaline in 11 of the 12 epileptic subjects along with the differential electrocortical patterns of their EEG reaction to mescaline.

Diagnostic and psychological aspects.—Hovey & Kooi (87) compared three groups of subjects (50 epileptics, 54 nonepileptics with organic brain disorders, and 65 nonepileptic, nonorganic patients who had psychiatric diagnoses) on their nonanswer responses (NR) to intelligence test items and on the presence of paroxysmal activity in their EEG. A significantly higher proportion of the epileptics (44 per cent) showed NR's than did those with organic brain disorders (17 per cent) or psychiatric diagnoses (9 per cent); 42 per cent of those showing paroxysmal activity on the EEG and NR's, and 52 per cent of those showing NR's had paroxysmal activity in their EEG's. The high proportion of epileptics and persons with paroxysmal EEG's who showed NR (a transient deviation of thought processes from an established goal) is strongly suggestive that NR's are not principally related to anxiety but to epilepsy. The absence of NR's, however, cannot be taken to mean the absence of epilepsy.

Janz (89) takes issue with the belief that pyknopsy is a benign non-epileptic seizure syndrome in children that usually disappears about the age of puberty. His review of 163 cases emphasized the clinical and EEG equivalence of this syndrome with epileptic "absences" of the petit mal variety, the low incidence of spontaneous cures (16 per cent), the incidence of grand mal spells (53 per cent), and the importance of an early medical treatment regime.

Clark & Knott (33), though reconfirming that the wave and spike pattern of activity in standard EEG recordings is related to epilepsy, find that subclassifications of epileptic varieties via the typing of some spike and wave patterns are only of limited assistance and that no true pathognomonic EEG indices are as yet available.

In addition to visual, auditory, olfactory, vertiginous, epigastric, masticatory, and gustatory auras, Macrae (113) has observed fear and Gastaut (64) has noted certain alimentary behavior as part of epileptic seizures. The fear experienced was not a fear of the seizure or a fear resulting from an aura. It came without content or relation to prevailing mood, thought processes, somatic sensations, or the ongoing situation; it welled up unaccountably, abruptly, and soon became recognized by patients as a sign that the seizure had already begun. It was usually followed by loss of consciousness. Macrae stresses that recognition of this fear as an aura, representative of epileptic activity, is important because it may be symptomatic of focal cerebral activity frequently associated with serious underlying neoplastic or vascular pathology. The high frequency of temporal lobe involvement in these cases may have important implications for investigation of a potential cerebral physical basis of emotional reactions. Gastaut found that hunger may occur as an initial symptom of a psychomotor attack, either alone or associated with rhythmic chewing, salivation, or other types of alimentary sensations, or as a post-ictal phenomenon associated with psychomotor seizures, and that it appears to be closely related to foci of activity in rhinencephalic structures.

Rodin *et al.* (155) and Beck & Guthrie (16) explored the psychological significance of the aura by means of detailed interviewing, psychological testing, and hypnosis. Rodin and his colleagues record that when seizures were medically controlled there was often an exacerbation of psychological difficulties in the patients in the inter-ictal period. Beck & Guthrie point out that the aura tends to be associated with a contemporaneous build-up of strong emotions as well as to be related to certain aspects of the total personality of the individual. The aura, especially when its elaborations can be psychologically investigated, is more than the activity of an isolated brain area, it is a complex phenomenon resulting from total integrative activity of brain (30).

The relationship between temporal lobe electrocortical foci of disturbance and the incidence of psychic phenomena in seizures has been, and continues to be, continually reiterated by investigators (52, 64, 110, 155, 196). Feindel & Penfield (52), on the basis of direct stimulation of claustr-

amygdaloid gray matter during surgery for temporal lobe seizures in 155 patients, observed that stimulation of this area can evoke automatisms similar to those noted in psychomotor seizures; and that this claustramygdaloid gray matter is a diffuse projection system, similar to the brain stem reticular formation and the intralaminar system of the thalamus, perhaps capable of diffuse regulatory effects on other parts of the brain. The periamygdaloid area may also be involved in the process of memory recording and the maintenance of the normal conscious state, both essential factors in behavior mechanism. Weil's (196) note about depressive reactions in some temporal lobe seizures, the Liddell & Northfield report on intelligence increases following post-temporal lobectomy (110), and the somnolent phenomena noted in association with hippocampal lesions (121, 139) seem to fit well with these conclusions.

Treatment approaches.—(a) Ataractic Drugs: The published reports on the use of ataractic drugs with the convulsive disorders have been quite limited. Barsa & Kline (13) tried to use reserpine in the treatment of psychotics with convulsive disorders only to find a definite increased incidence of convulsive seizures during the first six weeks of therapy. The limited contribution which reserpine made in the series of case studies was primarily a result of the sedative effect of the rauwolfia alkaloid. Bonafede (21) gives a preliminary report of the responses of 78 female epileptic patients at Craig Colony to chlorpromazine therapy. These patients were characterized as being hyperactive, aggressive, hostile, assaultive, destructive, and occasionally showing furor states. Chlorpromazine reduced the need for anticonvulsant therapy in some cases, but in others it seemed to result in increased seizures. However, when the convulsive aspect was adjusted by means of anticonvulsive medication to effective seizure control, a marked improvement in the disturbed behavior patterns was found with chlorpromazine. Merlis (175a) found that symptoms of anxiety and agitation could be relieved in his series of 150 psychotic epileptics, but that in a small proportion of the group (2 per cent) chlorpromazine administration precipitated convulsions.

(b) Anticonvulsant Drugs: Many new preparations, some representing minor variations of existing anticonvulsant drugs, others quite new in their chemical structure, have been under investigation during this past year. The reports in the literature do not accurately reflect the amount of ongoing activity. Timberlake, Abbott & Schwab (188) report favorably on their experience with Mysoline (similar in chemical structure to the barbiturates) in reducing the frequency incidence of a broad spectrum of seizure patterns in 96 patients ranging in age from 12 to 69 years. Rudolf (161), however, is less enthusiastic about this drug. He raises the important methodological question that research into seizure control fails to take into account the variations in the number of seizures which a patient may normally show over a long range of time, rather than the brief time sample which immediate pre- and post-use of a new drug usually involves. When convulsion incidence in periods of time from 2 to 20 years was taken as the base for comparison, less

than 25 per cent of the patients in his study showed improvement as measured by decreased frequency of fits. Mephnesin (102), SKF No. 2599 (126), and Acetazolamide (Diamox) (7) have each shown definite promise in the preliminary reports on the criterion of decreased seizure incidence (in presence of reduced side effects as compared with anticonvulsant drug with which being compared). There has been a dearth of quantitative psychological and clinical data about other behavioral responses to these drugs either in the pre-, post-, or inter-ictal periods. Davidson & Berman (39), who used a capsule combination of sedative and central nervous system stimulating drugs (Phelantin), do briefly note that among those patients whose seizures were completely controlled, a third reported increased alertness and better behavior with this drug.

(c) Surgery: Major cerebral ablation, such as hemispherectomy, has been used cautiously in the past, for obvious reasons, but more recently there has been a tendency to resort to this technique when confronted with problems where hemiplegia, poorly controlled or uncontrolled seizures, and mental abnormalities attributable to the malfunctioning of one sphere are present concurrently (and usually since infancy). Uecker, French & Johnson (191) studied seven subjects, ranging in age from 13 to 35 years and in intellect from idiocy to low-normal. Patients were individually tested for intelligence, and rated on the Vineland Social Maturity Scale pre- and post-operatively. Intellectually none of the subjects fell below the pre-operative level, with two showing some improvement (not controlled for decreased amount anticonvulsant drug). The post-operative institutional adjustment did not differ much from the pre-operative level. In a later study (61) the Minnesota group report on nine other patients who rapidly regained pre-operative levels, or better, of motor function; scored slightly higher in post-operative testing on psychometric examinations; and showed definite improvement in personality adjustment. Munz & Tolor (128) report essentially the same findings in their group of four patients who underwent hemispherectomy: slight, though statistically insignificant, increases in IQ; not much change on the Rorschach or Bender-Gestalt test; but an increased awareness of being sick, disabled, and bodily mutilated; with a heightened feeling of morbidity and depression. Seizures, as in the cases of the other studies, were better controlled.

A less radical operation, temporal lobectomy, has been utilized for psychomotor epileptic seizures which have not responded to anticonvulsant drugs and where EEG evidence of a focus of activity in the temporal lobe was present. Meyer & Yates (123) found no immediate change in intellectual level among nondominant lobe operees. Among the dominant lobe operees, there was a general decrease in both verbal and performance material. Patients retested after a year showed only a slight aphasic difficulty but had a marked deficit in learning ability. Falconer *et al.* (51) report that 26 of their 31 patients treated for intractable temporal lobe epilepsy by resection of the involved lobe showed marked improvement after surgery. Although no

psychological test results are included in the article, the authors claim that there was no apparent impairment of memory or intellect, although a third of the patients in whom the dominant lobe was removed had a slight but persistent dysphasia.

THE PSYCHOTIC REACTIONS

Schizophrenic reactions.—Although not a research report, Arieti's *Interpretation of Schizophrenia* (8) represents an important integrative contribution, from a psychoanalytically oriented approach to the clinical understanding of schizophrenic reactions. The author is convinced of the greater scientific productivity of psychological hypotheses over organic approaches in providing research leads to the etiology, diagnosis, processes, and modification of the schizophrenic reactions. The first four parts of the book are devoted to his exposition of the psychological considerations which have led him to believe that the basic process in the schizophrenic reaction is an impairment of the ability to abstract. This cognitive impairment is associated with severe anxiety in childhood which, through continuing or reexperienced trauma, leads progressively to a regressive retreat into archaic forms of thinking, desocialization, and emotional detachment. The historical review of the changing concepts of schizophrenia (Part I) is followed by an exposition of the psychodynamics of schizophrenia (Part II) which also includes a critical examination of the various psychogenic factors which have been given etiological importance. The very lucid discussion of the formal mechanisms which he considers to constitute the psychological structure of the schizophrenic reaction (Part III) is followed by a longitudinal view of the schizophrenic reaction from its initial anxiety stage to the final regressive level in which primitive oral habits, pain analgesia, and progressive psychological disintegration prevail (Part IV). The central nervous system involvement, the role of the cardiovascular apparatus, the endocrine and metabolic changes, and the other somatic changes associated with the schizophrenic reaction are critically evaluated (Part V) as psychosomatic aspects of the psychologically initiated disease process. The final section of the book (Part VI) presents the treatment efforts, emphasizing the contribution of psychoanalytically oriented psychotherapy and critically assessing the physical therapies.

Etiology and diagnosis.—The role of disordered metabolism or metabolic factors in the etiology of the schizophrenic reactions, especially those substances which contain an indole nucleus in their chemical structure, has been previously discussed in the section on induced behavior pathology. Freeman *et al.* (59) compared adrenal cortical function in 33 aged schizophrenics with that in 34 normal aged and found that elderly schizophrenics showed the same differences from elderly normal subjects that young schizophrenics did from matched young normals. These findings raise an important question for those investigators who have been proceeding on the hypothesis of adrenocortical exhaustion as an etiological basis of the schizophrenic reaction.

The conception of a schizophrenogenic mother who is generally described as over-solicitous, over-anxious, and domineering was not confirmed by an observation study (86) made of 22 mothers of young, male, single, schizophrenic patients during their visits to their hospitalized offspring. Only three of the visiting mothers could be so characterized, whereas the other 19 were classified as "sweet indulgent" (eight), "seductive" (one), "intent controlled, quietly purposeful" (three) and "removed, nonparticipating" (seven) mothers. Roberts & Myers (154) examined the characteristics of the schizophrenic reaction of the youngest male child of lower middle class families and found that this group showed a high incidence of distortions of the mother-child relationship, tension and poor communication between parents, and submissiveness on the part of the patient toward the other siblings.

Hoffer & Parsons (83, 84) object to the point of view that the differential diagnosis of the schizophrenic reaction, especially in its incipient phases, has to be primarily a subjective procedure. They contend that, as a group, schizophrenics have been shown to differ from other diagnostic groups in their pathophysiology as well as in their psychopathology. They cite the differential reactivity of schizophrenics in their blood pressure and leukocyte response to atropine, in their peripheral tissue utilization of oxygen, in their ability to convert sodium benzoate into hippuric acid, and in their physiological responses to a variety of stress tests. Using a battery of such tests and a scoring procedure, the schizophrenic members of a mentally ill patient group of 44 were satisfactorily sorted out.

Hirschstein (81) utilized the same basic conception discussed above to try to derive a prognostic index of spontaneous remission for hospitalized schizophrenics. The Funkenstein test (epinephrine for sympathetic nervous system stimulation, mecholyl chloride for parasympathetic stimulation) for autonomic nervous system reactivity was given to 60 schizophrenic patients who were then observed for several months and rated twice weekly on the Gardner Behavior Chart. It was Hirschstein's conclusion that blood pressure reactivity to a cholinergic drug could be of value in predicting which patients will improve spontaneously without intervening shock therapy, as well as in pointing to such patients who need some kind of physical therapy.

The diagnostic identification of that underactive group of patients who clinically show few if any clear-cut schizophrenic symptoms and who may go unrecognized even after years of study is the object of a new Rorschach index, the alpha score, proposed by Piotrowski & Berg (142). The basic hypothesis underlying the alpha score is that consciously or unconsciously, certain seriously ill patients become cognizant of the extent of their illness and develop habits of restraint and caution, underactivity, and overcontrol, as a security operation. Applying this index to a group of 100 schizophrenics and 45 neurotics, 83 per cent of the 70 patients who met the criterion of applicability of the index were correctly identified (46 schizophrenics with 3 plus alpha scores, 12 psychoneurotics with 3 minus alpha scores).

Fisher (54) describes the use of the Rorschach in the diagnostic identifica-

tion of another group, the ambulatory schizophrenic, who also tend to be a difficult problem in clinical diagnosis. The psychogram characteristics of the Rorschach performance of 10 such patients are presented as reflecting the intellectual strength which is available to the ambulatory schizophrenic as a defense means for covering up his lapses in reality response. Both pathology of thought and positive assets are present in the records of such patients.

Axel (10), in his analysis of the common characteristics of 10 patients who could be described as pseudoneurotic schizophrenics (according to Hoch & Polatin's criteria), points up the diagnostic problems in a third group that presents clinical difficulties in diagnosis. He stresses the autistic and dereistic life approach, the withdrawal from reality, the widespread and diffuse ambivalence, the pan-anxiety and pan-neurosis, the inability to associate freely, the occasional presence of micropsychotic episodes, the absence of accessory schizophrenic symptoms, and the inconspicuous regression. If the pan-neurosis fails to absorb all the patient's anxiety, then the patient experiences a catastrophic reaction which leads him into overt schizophrenia, according to Axel.

Psychopathological aspects.—Fitzherbert (55) reports three cases of schizophrenic reaction (in adolescent girls) which was associated with a marked increase in IQ at the onset of the illness. Even when factors which are associated with psychometric variation are taken into account, the marked temporary increase raises the question as to whether an early effect of the schizophrenic reaction may not be a facilitation rather than a blocking of associations.

Fellner & Weil (53) compared the responsiveness to therapy of 31 patients (IQ range 75 to 85) with that of a matched group that differed only in being of average intelligence. The manifestations of the schizophrenic reaction in the "dullard" group were not essentially different from those in persons of previously normal intelligence. There was a marked difference, however, in the therapeutic response to insulin therapy, and the authors suggest that mental dullness may act as an impairment of defense mechanisms so that the dullard is unable to cope with his psychosis as efficiently as the individual of normal intelligence.

Rosen (158) believes that the schizophrenic uses language in a continuum from pure communication, through communication mixed with defense, to pure defense. The schizophrenic's problem with language is further complicated in that he is having unfamiliar feelings and experiences which are difficult to describe. Another function for psychotic language may be its use for the discharge of unbearable tensions.

Straus & Griffith (179) were able to demonstrate both by the use of sodium amytal and some simple psychological maneuvers (e.g., tossing a ball) that a catatonic stupor could be temporarily reversed into action. The authors question Kahlbaum's assertion that catatonic behavior represents disturbed motor behavior. They prefer a psychomotor hypothesis for the catatonic disturbance, that there is an impairment of action in which the

cataleptic position reflects a perseveration of attitude, an inability to initiate change which may have significant determinant biological features.

The sedation threshold is an objective pharmacological determination which depends upon the EEG and the speech changes produced by intravenous amobarbital (Amytal) sodium. It has been shown to measure the degree of manifest anxiety in psychoneurotics. Shagass & Naiman (169) were interested in determining whether the sedation threshold could be used to measure manifest anxiety in schizophrenics, as well as in testing the hypothesis that impairment of ego functions, such as decreased reality contact, would raise the sedation threshold. Their experimental psychotic group included 11 with organic psychoses, 11 with acute schizophrenia, 22 agitated depressives, and 34 chronic schizophrenics. In the psychotic group as a whole there was no statistically significant correlation between the sedation threshold and clinical appraisals of degree of tension or manifest anxiety. The prediction of higher threshold for the chronic schizophrenics over apparently equally tense appearing acute schizophrenics was confirmed. They felt that their results pointed to a positive correlation of sedation threshold with degree of manifest anxiety and a negative correlation with degree of impairment of ego functioning.

Treatment of the schizophrenic reactions (80a).—One hundred consecutive schizophrenic patient admissions in 1920 were compared with a similar group of 100 schizophrenic patients in 1940. Freyhan (62) noted that the 1940 patients showed not only a shorter stay in the hospital but a higher rate of separations. If the patients who died are excluded, however, 40 to 45 per cent of both the 1920 and 1940 group became chronic, which is the same figure which Bleuler found to become chronic 50 years ago.

O'Reilly (135) has come up with a variation of the Harrower-Erikson Multiple-Choice Rorschach which he calls the "Objective Rorschach." Each of the 10 cards has 12 choices (4 normal, 4 neurotic, 4 psychotic), and the subject's task is to choose the two best answers. Criteria for improvement under therapy, clinically, included a loss of hallucinations and delusions, increased sociability and adaptability, and appropriateness of emotional responsiveness. The experimental group of 40 patients, tested pre- and post-therapy, showed that there was a shift of the Objective Rorschach score from psychoticism towards "normality" where clinical assessment indicated improvement, and increased psychoticism on the Objective Rorschach score when the original pathological syndrome became exacerbated.

Chemotherapy.—West and his colleagues (200) made a 14-year follow-up study of 781 schizophrenic patients who had received insulin coma treatment. Immediate results were 67.7 per cent improved or recovered; of this group, 334 or 63.3 per cent had a relapse. A second course of insulin treatment effected improvement or remission in 52 per cent of 122 patients who had relapsed. The most favorable prognosis with insulin coma therapy was associated with such factors as: (a) age over 16; (b) psychosis of less than six months duration with a clinical picture of paranoid, catatonic, or

undifferentiated schizophrenia; (c) a gain of more than 30 pounds in weight during treatment; and (d) at least 30 to 60 hr. of insulin coma therapy. Insulin coma therapy, though it may be effective in restoring a certain proportion of patients with schizophrenic reactions to their pre-psychotic level, does not lead to a permanent resistance to schizophrenia. Psychotherapy, environmental manipulation, and social and vocational rehabilitation do not diminish in importance but rather increase as the favorable response to insulin coma therapy makes the patient more accessible to reality.

Hoffer & Parsons (84) did an experimental clinical trial of histamine biochemotherapy with 12 patients diagnosed as having schizophrenia by the following 10 criteria: (a) physical sensation with disassociation, (b) delusions regarding physical objects; (c) delusions regarding others; (d) feelings of physical isolation and personal unreality; (e) rapid fluctuation of affect and inappropriate affect; (f) a feeling of having changed; (g) speech disturbance and intellectual blocking; (h) uncontrolled repeated interrupted and anxious thoughts; (i) ideas of reference, bizarre paranoid ideas, and (j) seclusiveness maintained or increased in hospital after one month's hospitalization. The follow-up study revealed that only four patients retained their improvement and those patients who were below 25 years of age seemed to benefit most from the histamine therapy.

Since its introduction in 1952 chlorpromazine (better known by its trade name of Thorazine) has had a tremendous impact in the United States upon the management of neuropsychiatric patients, especially those who are hospitalized. It has been conservatively estimated that more than 4500 reports on chlorpromazine have already appeared in the world's medical literature. Overholser (175a), in summarizing a symposium on *Chlorpromazine and Mental Health*, assessed the impact of this new drug as follows,

... chlorpromazine has an effect not only on the patient but also on the doctors and the attendants, the people who come in contact with the patient. Even though these people are not taking chlorpromazine, the fact that their patients are taking it and are, therefore, so much more accessible means that the doctors, nurses, and attendants ... can adopt a more therapeutic attitude toward them. It is a fact that in mental hospitals the disturbed wards have virtually disappeared.

And,

... with chlorpromazine we are going to be able to learn more about the psychopathology of the patient because he is able to discuss, without becoming disturbed about it, what has been going on in his head.

Chlorpromazine has been shown to be capable of modifying almost any state of psychomotor excitement without producing narcosis or coma; in many patients it increases the capacity to respond to psychotherapy (108, 175, 205, 208). It has shown itself to be effective in manic states, catatonic excitement, depressive agitation, toxic delirium, and other acute psychotic reactions (72, 106). Acute psychotic episodes tend to be quickly terminated, with a rapid reduction of aggressiveness, diminution of psychomotor activity and, in some paranoid and catatonic schizophrenics, with more logical

thinking, increased insight, and distance from hallucinations, even though these hallucinations may continue to be present (56, 144).

Other therapies.—Jackson (88) re-emphasizes the special problems associated with intensive psychotherapy of schizophrenics, especially those intrinsic to the therapist's personality. The management of his own anxiety in its various manifestations continues to be the central problem which must be dealt with by the therapist if he is to work with the schizophrenic patient. Grauer (75) reviews the various psychotherapeutic efforts which have been reported on with schizophrenics and concludes that it is premature to raise the question as to whether any presently known form of psychotherapy can "cure" the schizophrenic reaction. He focuses attention on the kinds of behavioral change which have been effected in the schizophrenic reaction, the need for better criteria of selection of patients who will benefit, and the need for research on the patient-therapist interaction.

Lighthart, Johnston & Sussman (111) report that the use of intravenous nikethamide (Coramine) combined with electroshock therapy for 13 schizophrenic female patients resulted in the need of fewer EST (electroshock treatments) for the experimental group, as compared with a matched control group, and also in statistically significant improvement on the Wechsler Bellevue Form I and on the Bender Gestalt Tests. They hypothesize that Coramine-EST brings about an increased cerebral blood flow leading to thalamic stimulation.

The affective psychoses.—Shagass (169a) measured the EEG responsiveness to photic driving at 10 flashes per second and at 15 flashes per second and found that, using the 15:10 response ratio, the response ratios were significantly higher in anxiety states than in depressions. In a follow-up study with normal subjects studied over a period of months, the quantitative EEG response to intermittent photic stimulation showed correlations with the day-to-day fluctuations in feeling: anxiety was associated with relatively greater driving at faster rates and depression with relatively greater driving at slower rates. The hypothesis of a "cortical excitability cycle" associated with the affective state is given support by Ludlum (112a) who compared patients with affective psychoses when ill with their remission states on a wide range of physiological indices. He found that the physiological indices of the affective psychosis group were within the normal range when in remission, but displayed patterns of subnormal or even immaturity level values in the sick state. He postulates that the physiological instability associated with the affective psychoses reflects an inability to maintain physiologic adaptation under stress.

Gutheil (78a) extends the notion of pseudoneurotic psychoses to a group of patients whom he has followed over long periods of time (up to 20 years). These patients show a variety of fears, hypochondriasis, and repeated episodes of anxiety which finally become clear-cut depressions. It is his opinion that there is a clear-cut, recurrent, functional psychosis from the beginning but that it masquerades behind neurotic manifestations until these defenses gradually give way.

The modification of electroshock therapy by the use of pipradol (Mera-tran) just prior to electroconvulsive treatment (6a) [by succinylcholine and pentothal sodium (149a) and by reserpine (40)] all seemed to result in higher effectiveness with many types of refractory depressions. There were fewer complications, a diminished number of total ECT treatments, and diminished need for maintenance treatments in the patient series studied. Rudolf (155a) reports that methylamphetamine, of the various sympathomimetic preparations studied, seemed to give the most rapid results, produce the greatest motor activity, and have the longest lasting effects upon depressions. Of the 219 severely depressed patients he studied, 183 improved: 79 markedly, 81 moderately, and 23 slightly. Unfortunately, like all too many studies reported in this area, there is a lack of reported systematic behavioral observation and careful investigation of psychopathological processes that might enable one to go beyond the mere statement of final outcomes.

PSYCHOPATHIC PERSONALITY REACTIONS

Antisocial behavior.—A multiplicity of hypotheses about the etiology and "basic intrapersonal dynamics" which lead to the formation of the delinquent character (34, 60, 69, 97, 166) seems to be quite characteristic of this field. Schulman (166), for example, places the delinquent's acting out effort to deal with his anxiety at the core of his theory. The affectionless developmental setting in which the delinquent's impaired ego had its origins fosters his constant use of primitive, infantile methods of coping with his anxiety. Schmideberg (165) prefers an hypothesis of an inconsistent superego with its consequent defects in integration to that of the developmentally deprived, impaired ego. Dalmau (38) postulates the existence of an overstrict superego that produces a gradual persistent repression of normal aggressive drives with a consequent accumulation of destructive psychic energy as the basis for antisocial behavior reactions. According to Michaels (124), a biological deficiency (e.g., a specific psychosomatic disposition such as persistent enuresis) interacting with psycho-social forces can become a dynamic prototype for the undercontrolled, immature, impulsive, pleasure dominated behavior which is characteristic of a certain pattern of psychopathic personality.

Karpman (95) is highly skeptical as to whether any "delinquent personality" exists. His proposal of three general types of delinquent behavior reactions, (a) that associated with brain pathology, (b) that reflecting pathology of intra-familial relationships (166, 203), and (c) that representing social dislocation or disorganization (129), does not include such considerations as constitutional factors, emotional traumas, and genetic factors, but yet does provide a more systematic basis for ordering the data about delinquent behavior reactions than the "character trait" hypotheses. Jenkins (90) extends this categorizing to include the outcome of the delinquent behavior, i.e., whether it is adaptive or nonadaptive.

Addiction behavior reactions.—Winkler *et al.* (205a) believe that there has been a tendency in the literature to overestimate the cause-effect rela-

tionship between alcoholism and criminality. Classifying their case material into primary alcoholism associated with various psychopathological states, and acute alcoholic intoxication, they found that primary alcoholism was but rarely associated with a major delinquency. Acute alcoholic intoxication, however, was found to be a precipitating cause in many cases of serious crime. Krauweel (103a) found that for 20 per cent of his 564 penal subjects there was a relation between the last offense and alcohol, while in the psychopathic hospital, only 16.5 per cent had such a relation. Alcohol was not used by 68.8 per cent of the patients in the psychopathic hospital, whereas only 19 per cent of the prison inmates used no alcohol. The psychopaths showed the highest percentage of nondrinkers as well as the highest percentage of heavy drinkers among the alcoholics.

The results of a five year multi-disciplinary study of the etiology of chronic alcoholism are presented in a monograph edited by Diethelm (42a). On the basis of her study of the underlying psychopathology and personalities of 161 cases of chronic alcoholism, Sherfey concludes that alcoholism is not a single entity or a disease, but a symptom associated with several illnesses or syndromes. An adequate differential diagnosis is necessary for the understanding of each case. Fleetwood's provocative report concerns the consistently higher presence of a "resentment" substance in alcoholic patients versus nonalcoholic, and the significant reduction of this substance after alcohol intake by alcoholic patients and not by the injection of any of the other drugs investigated. Emotional tension, and the "tension substance" as well were reduced more significantly by alcohol in the alcoholic than in the nonalcoholic subjects. The study of family and personal backgrounds led Bleuler to conclude that there is no evidence that alcoholism causes epilepsy or oligophrenia in the offspring, that there are important endocrinological factors in alcoholism, that the genetics of alcoholism is very closely related to the genetics of other abnormal personality development, and that alcoholism has to be considered mainly as a symptom of abnormal personality development.

While there may be no typical alcoholic personality prior to alcoholism, Wellman's (199a) study indicates that there are many characteristics and experiences which are common to those who become alcoholics. In his group of 26 male excessive drinkers, all under 35 years of age, there was not one who began to drink or to get drunk because of personal problems; rather, social empathy seemed to be the most important single element in the development of excessive drinking. Unlike alcoholism, intoxication for the drug addict often facilitates many aspects of living; it does not interfere with motor coordination and the ability to function in ordinary tasks. The alcoholic has reason to fear his intoxication; the opiate addict has reason to fear not intoxication but, rather, abstinence. Gerard's opinion (66a) is that there is far more basic similarity in the personality structure and needs of alcoholics and opiate users than basic difference and that personality factors per se are of minor importance in determining which path is taken to the same goal. Social, cultural, and historical factors determine the choice of

drug. The development of an intolerance to alcohol after a long history of excessive drinking, especially in relatively young men, needs careful psychological and neurological assessment. Tumarkin *et al.* (188a) report the cases of seven males, mean age 32, with an average history of excessive drinking for 11 years, who showed lowered digit-span and digit-symbol performance on Form I of the Wechsler-Bellevue Intelligence Scale, abnormal EEG's, and diffuse cortical and subcortical atrophy, particularly in the parietal region, on pneumoencephalography.

Management of the acute, subacute, and post-alcoholic states has been markedly improved with the advent of the new ataractic drugs, chlorpromazine, miltown, and sparine (51b, 127a, 187a). Not only have these drugs helped in the modification and control of the excitement phase, but they also contribute to the patient's readiness to enter and remain in psychotherapy, according to Thimann & Gauthier (187a). Recognition of the impact of the chronic alcoholic male upon his family organization and way of life (88a) has led to some experiments in group therapy in which both the alcoholic husbands and their wives participate in separate, though concurrent, groups. Gliedman *et al.* (69a) report that of an experimental group of nine alcoholics and their wives, five improved on the drinking scale, two abstained completely, and two modified their drinking pattern somewhat. The patients who did not improve or improved only slightly were those whose self-descriptions disagreed most with their wives' descriptions of them.

Gerard & Kornetsky (66b), using the Rorschach, Human Figure Drawing Tests, IQ tests, and psychiatric interviews, compared 33 hospitalized adolescent opiate addicts with 23 control subjects of similar age, ethnic background, and exposure to illicit drug use. The addicts showed statistically significant and clinically impressive more personality malfunction than the controls. The young addicts tended to show certain denial of limitations of their socioeconomic status, wish-fulfilling distortions of reality, and orientation towards status goals rather than towards goals of satisfactions and security. McLean and others at the Lexington Hospital (117a) have shown that certain psychological measures of verbal intelligence and degree of manifest anxiety could be used as significant predictors of acceptability for psychotherapy of institutionalized narcotic addicts. The rigors of the withdrawal syndrome have been dramatically reduced or eliminated by the use of promazine (sparine) (51a, 127a) and chlorpromazine (3, 59a). The potent antiemetic action of these drugs, their hypnotic and sedative effect upon the central nervous system so that psychomotor excitement and tremors were controlled, the absence of mental confusion during waking hours, and their tension-relieving effect facilitated the establishment of rapport with therapists and rendered the patients more amenable to psychotherapy.

PSYCHOSOMATIC REACTIONS

(Psychosomatic medicine) . . . is not a direct line derivative of the psycho-soma preoccupations of our medical elders. . . . Present-day psychosomatic medicine is

historically and actually unique. . . . It constitutes a movement to counter-balance and to correct some of the erroneous and corrupting ideas and viewpoints propagated in organicist medicine.

Galdston (63) goes on to warn workers in this area that they have begun to succumb to the twin sins of organicist medicine, namely, the error of specificity and the error of time-sequential-causality. Clear understanding of psychosomatic reactions can come only if the organism is viewed as a dynamism continuously exposed to the effects of the multiform forces which surround it, an organism which is affected and determined by the very modification it experiences as a result of the interaction of its own dynamism with these surrounding forces.

Some force is given to this conception by such studies as those by Reiser, Reeves & Armington (145) which report that differences in the experimenter-subject relationship may alter the total meaning of the experimental situation so that different psychological and physiological mechanisms of response are evoked by an otherwise identical test procedure (blood pressure, heart rate, ballistocardiogram measurement).

Starr (177) proposes a fourfold classification of reactions to replace the overly broad term psychosomatic reaction: (a) psychological "pseudo-somatic" reactions, in which there is no demonstrable physiological dysfunction or patho-structural change (e.g., conversion hysteria); (b) psychophysiological reactions (e.g., anxiety states) in which there is a close correlation between emotional states and the concomitant physiological changes; (c) psycho-pathophysiological reactions (e.g., hypertension) in which psychological forces precipitate, exacerbate, or perpetuate structural changes in an organ; and (d) somato-psychological reactions in which psychological dysfunctions appear secondary and incidental to physical disorders.

Livingston (112) reviews some brain stem mechanisms relating to psychosomatic reactions and systematically organizes and classifies the research in this area.

Pain is one of the fundamental psychosomatic reactions. Szasz (183) suggests that "pain" may be distinguished at three levels of symbolization, arranged in a hierarchy of increasing complexity: (a) the biological level, the signal value by which the perceptive part of the organism registers the threat to structural or functional integrity; (b) the communication level of pain involving more than one person where the problem of communicating the experience of pain to another person is involved and the expression of pain is a fundamental method of asking for help; and (c) the third level where pain no longer denotes a reference to the body but to a symbolic transformation.

Gastrointestinal reactions.—Sixteen of 25 obese patients referred to a special clinic for weight reduction were found by Stunkard *et al.* (180) to have a distinctive syndrome of nocturnal hyperphagia, insomnia, and morning anorexia. Another four of this group of 25 showed this syndrome with minor variations. Past weight reduction efforts had not met with much success. The syndrome seemed to be closely related to periods of stress which

are intimately related to the overeating which led to the obesity in the first place. Frazier *et al.* (56a), in reviewing 15 years of study of children's eating difficulties (with consequences ranging from obesity to skinniness), come to the conclusion that the child's need to comply with parental wishes in the area of food intake was one of the major determinants in the production of the specific gastrointestinal reaction which the child developed.

Some of the neat specific formulations (to which Galdston objects so vigorously) which causally relate personality characteristics with gastrointestinal symptomatology do not seem to hold up very well under experimental conditions. Wolff & Levine (206) measured nocturnal gastric secretions of ulcer and nonulcer patients under a situation of experimentally induced mild stress. The acid level of the nonulcer group showed a marked increase with stress, whereas that of the ulcer group did not. No significant differences were noted in verbal or other behavior between the groups. It is possible that an already high level of persistent anxiety in the ulcer group was not much affected by the additional external stress introduced experimentally, but there is also possible the interpretation that the gastrosecretory mechanism of ulcer patients does not respond in as simple a correlative manner to security threat as has been hypothesized.

Minski & Desai (127) evaluated the performance of 25 male peptic ulcer cases and 25 diagnosed hysterics on a battery of psychological tests, which included the Rorschach, the Shipley-Hartford Retreat Scale, the Porteus Mazes, and the Rosenzweig Picture-Frustration Test, and a clinical interview. There were significantly more subjects in the ulcer group who were found to be neat, tidy, methodical, and conscientious than among the hysterics, but from one-half to one-third of this peptic ulcer group did not exhibit the personality traits usually attributed to the so-called "ulcer personality." Neither the clinical assessments nor the psychological test results led to a classification typing into one or two categories of "ulcer personality." In 43 out of 45 men of different professions, ages, and education, all with radiologically-verified peptic ulcer, first symptoms started after isolation from a community which had been experienced as a protective one.

Pflanz *et al.* (140) point out that patients could often withstand panic and malnutrition if they were in harmony with their community, but had complaints even on a bland diet if they were not. This community might be that of the family, that of professional equals, or a symbolized organization in which efficiency was the mark of belongingness. Recurrence of symptoms in such patients was closely related to feelings of estrangement or isolation from any of these "nurturant" communities.

There have been in the past a plethora of individual case reports and psychoanalytic writing on the alternation between psychoses and psychosomatic disorders (especially peptic ulcers). Although such alternation may occur, Honig's (85) study of postmortem findings in 37 psychotic patients over a four-year period, as well as his survey of a total hospital psychiatric population of 800, showed that 22 (2.8 per cent) had dual diagnosis of peptic ulcer and psychosis. In half of this group the psychosis manifested itself

first, and with the appearance of the peptic ulcer, the psychosis became more prominent. Giovacchini (68), though dealing with another type of problem, raises a similar question to that which is implied in Honig's findings about "specific dynamics" in psychosomatic reactions. A review of cases showing psychosomatic disturbances led him to the conclusion that multiple psychosomatic entities in the same person (e.g., hypertension and peptic ulcer, hypertension and asthma, etc.) are not uncommon. Tension from a particular conflict may express itself through more than one organ system, affecting first one, then another later, or both concurrently. This goes considerably farther away from specificity formulations than even the psychoanalytic compromise of "conditional specificity" of Weisman (199).

In a somewhat unusual setting, Karush, Hiatt & Daniels (96) observed and recorded the physiological activity of the parotid glands, the peripheral-vascular bed, and the distal colon in six patients with ulcerative colitis during psychotherapeutic interviews. The concurrent recording of the interview then permitted time matching of physiological and psychological productions. Fear, as verbally rated, was the emotion most prevalently associated with coincident colonic activity. Vascular bed responses were not specific but seemed to be related to fear and anxiety. Colonic activity was associated with inhibition of salivary activity which was almost completely absent during the interview. Engel (46) summarized the psychological data of his 39 patients as well as the published reports on more than 700 patients with ulcerative colitis as showing (a) defects in personality structure long antedating the onset of colitis, (b) a characteristic type of dependent and restricted relationship with people, (c) consistent psychopathology in the mothers, and (d) failure to achieve full heterosexual development.

Cardiovascular and circulatory reactions.—There has been a marked tendency for chemotherapy to displace psychotherapy in the approach to hypertensive problems (as reported in the literature during the past year). Shapiro *et al.* (170, 171) demonstrated that the study of the various chemotherapeutic agents could not be carried out without a better understanding of variations in blood pressure resulting from the psychological impact of the medication and the therapeutic regime, the attitude of the physician toward both the patient and the drug, and the life situation of the patient and his reactions to environmental situations. Alternation of drug and placebo clearly indicated that improvements were frequently independent of whether it was drug or placebo which was being administered, that blood pressures tended to fall or rise as the physician's enthusiasm for the drug was up or down and with various other manifestations of responsiveness to the interpersonal relationship.

Miles *et al.* (125) compared 46 male patients having coronary disease with 49 controls free from cardiovascular pathology in terms of specific personality traits and failed to find convincing evidence of any significant differences which could be implicated in the genesis of coronary atherosclerosis. Although coronary patients tended to work harder, showed less tendency to introspection, and more difficulty in handling their aggression than the

controls, the similarities between the two groups were far more impressive than the tendencies to differ. These findings do not minimize the consideration of personality factors in the management of the coronary disease, once the cardiac damage has occurred.

The blood circulation aspect of emotional response has long been recognized, but it has been technically difficult quantitatively and accurately to assess this aspect and correlate it with psychological occurrences. Ackner (1) measured the pulse-volume of 13 anxious subjects, 12 in-patients without manifest anxiety, and 10 controls during wakefulness and sleep (using EEG criteria for determining onset of sleep). The anxious patients showed the greatest pulse-rate decrease during sleep, presumably when a state of maximum relaxation prevailed. Graham (74) approached this same problem of peripheral vascular changes under emotional stress by studying four patients with Raynaud's disease (which is characterized by vasoconstriction of arterioles and smaller vessels during attack episodes). He found that there was a specific association of attacks of Raynaud's disease with the occurrence of strong hostility feelings and anxiety reactions in the patient. This vasoconstriction, though most prominent in the hands, also occurs in the skin elsewhere on the body.

Respiratory reactions.—Dekker & Groen (41) were able to reproduce asthmatic attacks in some of their subjects by exposing them to laboratory situations comparable to those which had led to this type of reaction in their past history. It was noted that in those cases in which the stimuli did produce asthmatic attacks, other stimuli sometimes led to intense emotional reactions but not attacks. Following the provocation tests, there was frequently an outpouring of emotional fantasies, traumatic experiences, and disturbed dreams. Leigh (107) reports a case where relief from an asthmatic attack was obtained during a psychotherapeutic session in which there was much outpouring of emotional material. After three such interviews the patient fell asleep spontaneously for the first time in many nights, but woke suddenly in an asthmatic attack and died within five minutes. Her lungs showed the characteristic pathological picture. Leigh proposes that excessive emotional discharge may have led to a parasympathetic counter-reaction and hypersecretion of mucous. He cites other incidences of sudden asthmatic deaths on psychiatric wards occurring some hours after psychotherapeutic interviewing and suggests the use of parasympathetic blocking agents (atropine) as a routine prophylactic measure. Alexander & Visotsky (4) present some studies of asthma in adults and children in which the crucial precipitating factor was believed to be a disturbance of the symbiotic mother-child relationship, even though the actual mother may have been substituted for by symbolic representations.

Goldensohn (70) and Ames (6) point up the intimate association of respiratory mechanisms with the reticular formation of the medulla and the limbic lobe or rhinencephalon. Both of these structures are considered to be of importance in the elaboration and expression of emotion, in the alteration of states of consciousness, and with reactions of fight or fright in the broadest sense. Just as anxiety and fear may activate and alter breathing

patterns, there exists a potential mechanism whereby alteration of the breathing patterns may influence the emotional state and the state of awareness (e.g., hypnagogic and hypnoidal phenomena).

Other psychosomatic reactions.—Studies of psychosomatic reactions associated with pregnancy continue to be characterized by over-simplified methodology and absence of control groups (25, 159). Cramond (36) is an exception in his matching of 50 women who had severe uterine dysfunction during recent labor with 50 normal labor controls. There seemed to be little or no psychological difference between the two groups other than a tendency for the uterine dysfunction group to be more suppressive of overt anxiety in unfamiliar situations, to be reserved, show some suspiciousness and some reluctance in talking about themselves. It is Cramond's conclusion that psychiatric methods of prediction and prevention in the antenatal period would have no effect on the occurrence of uterine dysfunction.

Both Gates & Weinberger (65) and Weckowicz (195) found that psychosomatic reactions involving urinary frequency, burning, or other disturbed function tended to have strong affective hostile-aggressive components. In Gates & Weinberger's series of 47 patients, 27 had ideas of being damaged, and their genitourinary complaints were strongly suggestive of "organ-bound psychoses." Weckowicz found that the 10 cases classified as functional tended to display their aggressiveness more freely than those who had had an organic basis for their increased frequency of micturition. More than half of the total group of patients scored high on the "neurotic triad" (hypochondriasis, depression, and hysteria) of the Minnesota Multiphasic Personality Inventory.

LeShan & Worthington (109) studied 152 patients with neoplastic disease and 125 control patients with other or no known disease by means of the Worthington Personal History Test. They discriminated three important factors: (a) loss of an important relationship for which no equally satisfactory substitute cathexis could be found before the diagnosis of tumor (E:72 per cent, C:12 per cent); (b) inability to express hostile feelings toward other people either verbally or in other ways (E:47 per cent, C:25 per cent); and (c) tension over the death of a parent, often an event that had occurred far in the past, with guilt or anxiety feelings or both evident (E:38 per cent, C:11 per cent). On a trial run of 28 new records, the authors correctly predicted 24 as to whether they had been written by cancer or noncancer subjects.

Reznikoff (147) studied a group of 50 patients who came to a breast tumor clinic, 25 of whom were eventually found to have malignant neoplasms while the other half did not. He also used a sample of 25 women from another clinic known to be completely free of breast pathology. The Thematic Apperception Test, a sentence completion test, and a heterosexual relations questionnaire were used. The cancer subjects used in this study differed significantly from women without breast pathology in more respects than from women with benign disorders. There was significantly less diversity between the normal and the benign subjects.

Wheeler & Caldwell (201) used the Kent E-G-Y, Rorschach, Draw-A-

Person Test, Family Preference Ratings, Directed Interview, and the Rosenzweig Picture-Frustration Study to compare 20 women with breast cancer, 20 with cancer of the cervix, and 20 normal controls. While suggestive differences between the breast, cervix, and normal subjects were found, the three groups were more alike than they were different. They corroborate Reznikoff's findings of more negative and conflictual feelings in the broad area of heterosexual behavior for the breast cancer patient. The cervix neoplasm group tended to be less controlled in their sensual and emotional responsiveness to external stimuli and were more sexually promiscuous and more preoccupied with sexual and body ideation. Bard (12) found that the Rorschach and Thematic Apperception Test were less adequate than a special psychiatric interview in preoperative predictions of postoperative invalidism.

ABNORMAL BEHAVIOR IN CHILDHOOD

The revised, rewritten, and original papers which constitute the *Psychopathology of Children with Organic Brain Disorders* (18) by Bender and her associates contribute to a fourth volume in a series which gives promise of becoming a comprehensive presentation of the psychopathology of children who need residential institutional study and treatment in order to meet life's demands. This volume deals with the responses and personality problems of children who have been frustrated by organic disorders of the brain. The neurological, psychological, and social aspects of cerebellar and cerebral lesions are discussed from diagnostic, theoretical, and clinical management points of view. Repeatedly Bender emphasizes that the behavior manifestations and prognosis for children with organic brain problems is far more dependent upon the way in which they are accepted and assisted than upon the presence or degree of the organic factors themselves.

Strauss & Kephart (178) go considerably beyond their first volume, which was primarily a report of research, to make a systematic, bold venture into theorizing about brain functions based on their continued investigations of the behavior of brain-injured children. The first six chapters develop the implications of such concepts as servomechanisms, scanning mechanisms, homeostatic mechanisms, and the theory of electrical circuits for the anatomy and physiology of the central nervous system as well as for the psychopathology of perception, language, concept formation, and behavior. A final chapter, "Essays on Mental Functioning," is a frankly speculative attempt to integrate phylogenetic and holistic conceptions of brain function to provide hypotheses for further research in this area. Goldenberg contributes a valuable chapter on the diagnostic psychological assessment of the brain-injured child with normal intelligence. In one brief chapter Lehtinen attempts to correlate some of the educational implications for the training and development of the brain-injured child with the theoretical framework presented in the rest of the volume.

A third major contribution to the understanding of organic brain pathology in children is the book edited by Cruickshank & Raus (37). Their selec-

tion of contributors goes a long way towards achieving the goal of a basic sourcebook for the multidisciplinary approach which is required for the diagnosis, treatment, and community adjustment of the cerebral palsied child. The chapter bibliographies, especially in those sections dealing with diagnosis and assessment, are somewhat less selective than the caliber of the chapters would have led one to expect. The reproduction of published norms furnishes the clinician with very useful diagnostic source materials.

Psychopathology of Childhood edited by Hoch & Zubin (82) is the published proceedings of the 44th Annual Meeting of the American Psychopathological Association. It is not intended to be a comprehensive review of the total field but rather a critical assessment of the research areas which have shown recent or current activity. Outstanding résumés of the status of the research and the problems in their respective fields are made by Benda & Farrell (Mental Deficiency), Bradley (Organic Factors), Goldfarb (Psychologic Deprivation in Infancy), Ackerman & Behrens (Child and Family Psychopathy), Lourie (Psychosomatic Problems), and Levy (Oppositional Behavior), to mention only some of the contributors.

The collections of papers edited by Caplan (29) and the reprinted articles which make up Volume X of *The Psychoanalytic Study of the Child* (45) are primarily centered around clinical case presentations and their implications for psychoanalytic metapsychology. *The Proceedings of the International Institute of Child Psychiatry* (29) also contain some research reports on parent-child relationships as they affect the child's adjustment to illness and hospitalization. Lindemann and Ross present an interesting study on the use of the Bales Interaction Process Scheme for analyzing children's doll play as a basis for making predictions about the child's emotional adjustment in school.

Diagnosis and etiology.—Considerable clinical experimental evidence has accumulated (82, p. 122) which indicates that many severe congenital defects may be produced in the intra-uterine organism by various stresses undergone by the pregnant mother. Cortisone has been shown to have multiple effects upon the developing embryo either by impact on the mother's endocrine balance, by the production of metabolites which may cross the placental barrier, or by influence upon the placenta itself. By inference from the action of cortisone, severe psychological stress in the mother may have important consequences by virtue of effects upon her own adrenal status. Thus, maternal psychological experiences may potentially have broad and enduring effects upon the developing organism.

Turner (189) describes a neonate syndrome of restlessness, fussiness, excess crying, loose stools, considerable vomiting, and unusual sensitivity to sound. A survey of 100 mothers revealed 12 babies whose behavior conformed to the above syndrome. Of the mothers of these neonates: one mother refused to answer, three were found to have illegitimate babies, two were having their first infants in a new land, one had had an illegitimate baby but her husband did not know this, and the remaining five admitted undue emotional stress or fatigue. Turner felt that prenatal emotional stress might af-

fect the reactivity of the fetal nervous system and alter the whole pattern of postnatal behavior.

Rogers *et al.* (156) compared 471 children, who had been referred to school clinical services because of some behavior problem, with 359 controls (the next alphabetically in the class). Age, socio-economic status, parental age, previous maternal pregnancies, and length of labor were found to be the same for both groups. The cases had a significantly higher frequency of premature births and abnormalities of prenatal and neonatal conditions (toxemias of pregnancy and bleeding during pregnancy). Hyperactive, confused-disorganized cases had a larger proportion of abnormalities in the pre- and neonatal periods than cases evidencing all types of behavior disorders.

Richmond & Lustman (148), using 31 infants three or four days old, demonstrated that there were qualitative and quantitative individual differences in autonomic function in the first days of life. They hypothesized that autonomic endowment may be one factor which predisposes the individual to somatic change and to specific psychosomatic disease. On the basis of twin-concordance studies, Kallmann & Roth (93) were unable to reach any conclusions as to the nature of the contribution of genetic factors to the occurrence of preadolescent schizophrenia. Bender & Grugett (19), however, were more willing to ascribe a positive role to hereditary factors in the etiology of childhood schizophrenia on the basis of a contingency study. Their comparison of 30 schizophrenic children with 30 nonschizophrenic children showed higher incidence of familial neuropathic traits, more frequent referral because of behavior disturbances in area of fundamental processes, and lower incidence of distorted family relations in the schizophrenic cases. They concluded that hereditary and endogenous factors were more important than exogenous factors for the etiology of schizophrenia in childhood.

There has been considerable caution in diagnosing manic-depressive psychoses in children, especially mania in the prepubertal period. McHarg (117) presents a carefully observed case of an 11 year old prepubertal girl in whom no physical findings suggestive of brain disease or schizophrenia were found. She was in a state of continuous elation, psychomotor activity, pressure of talk, and flight of ideas; was interested in her immediate environment; but had no insight into the fact that she was ill. The manic state was followed by a depressive phase which lasted eight weeks and which ended with a complete remission. The home was presumably stable, affectionate, and showed no environmental factors to account for the illness. There was evidence of manic-depressive psychosis in the family. Campbell (27) discusses his series of milder yet definitely cyclothymic disturbances in children who show hypomanic and depressive moods, fear reactions, psychomotor retardation, insecurity, crying spells, and somatic complaints. These children are rarely brought to treatment because of delinquent behavior and seem capable of forming warm relationships with peers and teachers. He agrees with McHarg that the evidence strongly suggests some sort of endogenous basis for this type of reaction.

Despert (82) reports that 68 children, in a series of 401 consecutive cases,

had a diagnosis of obsessive-compulsive neurosis of varying degrees of severity (e.g., marked interference with intellectual and social functioning). Contrasted with the schizophrenic child, there is little or no damage to the reality testing function. No matter how bizarre the obsessive thoughts or compulsive acts, the patient experiences them as alien to his personality. Characteristically there is a strong guilt reaction, and the author attributes the anxiety to unresolved oedipal conflicts rather than, as in the schizophrenic, to devastating emotional deprivation experienced in the pregenital phase.

Psychopathological aspects (71a).—Taterka & Katz (186) gave EEG and a battery of psychological tests, including the Rorschach, Bender-Gestalt, Human Figure Drawing, and the Wechsler Intelligence Scale for Children to 195 severely disturbed children. Their results were as follows: (a) a high percentage of EEG abnormality in schizophrenic children (organic factor); (b) no correlation between EEG and specific behavioral abnormalities (e.g., extreme passivity, hyperactivity, aggression); (c) the greater the cerebral dysfunction on EEG, the greater the likelihood that psychological test results would be abnormal, particularly those dealing with perception, perceptual-motor tasks, and the body-image.

Disturbance in communication has been frequently noted as a prime defect by workers with the schizophrenic child. This disturbance is most typically manifest in language failure. Wilcox (202) enumerates the various ways in which speech disturbance may be evident, e.g., mutism, infantile speech, neologisms, substitutions, generalizations, restrictions, disturbances of speaking tone and inflection, all of which may occur either singly or in combination. Norman (131) found that all of the 25 schizophrenic children in her series showed speech and language disturbance in some way. She considered the language disturbance to be another aspect of the withdrawal and avoidance behavior which the schizophrenic child shows in so many other ways, a pattern which tends to arouse considerable anxiety in those who have to interact with such children. Wilcox is impressed by the lack of fluidity and communication in the parent-child relationship, and he believes that the pathology in the speech sphere is a function of the parent's and the child's efforts to maintain a status quo in their relationship because both fear the potential effects of altering the existing balance which controls their coexisting feelings of affection and hostility towards each other.

Kanner & Eisenberg (93a) re-evaluated 63 autistic children at a mean age of 15 years, with a mean follow-up period of nine years. The original diagnosis had been made upon the pathognomonic presence of extreme self-isolation present in the first years of life and obsessive insistence upon the preservation of sameness. All had exhibited distortions of language function. Of the 63 cases, 34 were in full-time residential settings and 29 at home with parents or foster parents. The striking significance of mutism or lack of useful speech by age five is of prognostic interest since only one of the 31 without the ability to communicate verbally at five showed any improvement, whereas half of the 32 children who had communicable speech at five showed some improvement. Kanner & Eisenberg stress that none of the

varieties of psychiatric treatments seemed to have any noticeable effects, that inherent factors and the early decisive impact of the early constellation of the parent-child relationship have a determining effect upon subsequent development. Both authors agree that, while there may be some value in separating out early infantile autism, these children share the general features of schizophrenia, and the syndrome should be logically classified as one of the schizophrénias.

Treatment and behavioral modifications.—Gurevitz & Helme (78) gave pre- and postshock psychological tests, as well as a one-year follow-up, to 16 schizophrenic children treated with electric shock. The Revised Stanford Binet; the Non-Language Multi-Mental Test of Terman, McCall & Lorge; the Bender-Gestalt test; the Goodenough Draw-A-Person Test; and a controlled free-drawing called the Trees-Fence-Road Test were included in the test series. The findings were as follows: (a) intellectual efficiency was significantly reduced immediately postshock, but recovered or was slightly higher at the time of the one year follow-up testing; (b) logical reasoning seemed altered at follow-up compared with preshock state; (c) simple cognitive-perceptual organizing process was not grossly disturbed even within 48 hr. of shock; (d) drawing tests suggest release of drives and loosening or breaking of defensive barriers. ECT is viewed by these authors as a potential force for weakening of distorted but rigid personality structure and for the release of the pressures of drives so that a potential reorganization of personality becomes possible.

Clardy & Rumpf (32) are considerably less sanguine about the use of ECT (electroconvulsive treatment) than Gurevitz and the Bellevue Group. They review the experiences of 32 children, all under 12 years of age when admitted to the children's unit of a state hospital, who had all received ECT therapy. The common experience for those children diagnosed as schizophrenics and primary behavior disorders had been to have a temporary improvement which was followed by not only a regression to the preshock level of behavior but in some cases to an exacerbation of the abnormal behavior reaction. The authors feel that the effects of ECT on children are essentially temporary, resulting in no sustained improvement, and that ECT should not be given to very young children because of the lack of knowledge of the impact on developing brain of the ECT as well as of the traumatized attitudes of the children towards treatment.

The initial reports on the use of the ataractic and tranquilizing drugs in the treatment of behavior disorders in children (58, 66, 132, 174) have been generally similar to results obtained with adults. Gatski (66) used chlorpromazine with nine boys out of a group of 150 in a residential treatment center because of their high level of agitation and their chronic acting out behavior. He found that all children showed some improvement within one week after being placed on the drug. Not only did it seem to make the children more tractable, better able to adjust and conform, but it also seemed to facilitate their communication and relationship with the therapist.

Oettinger (132) used meratran with 47 children diagnosed as having be-

havior disorders. About half showed some improvement, a fifth did not change, and about one-fourth were made worse on the drug therapy. Oettinger believes that such positive effects as were noted seemed to be associated with the increased attention span, the decreasing irritability, and the consequent improvement in function.

Freedman, Effron & Bender (58) report an exploratory study of pharmacotherapy with 195 children between the ages of 7 and 12. The children were evaluated on the basis of eight categories of behavior: (a) motor activity, (b) manifest anxiety, (c) relationship problems with other children, (d) relationship problems with adults, (e) affect, (f) motility, (g) phantasy life, and (h) pathognomonic symptomatology. Placebo controls were used. Diphenhydramine (Benadryl) led to improvement in all the cases diagnosed as primary behavior disorders, as did mephenesin (Tolserol) in those cases with organic brain disease. In the schizophrenic cases, no drug was outstanding, though chlorpromazine led to improvement in 57 per cent of the cases. In the area of hyperactivity, best results were obtained with chlorpromazine (41 per cent); diphenhydramine served best for manifest anxiety (54 per cent improved) as well as in alleviating depressed affect and in improving relationship problems with adults. Silver (174) endorses the use of drugs in the management of childhood schizophrenia, especially the antihistaminics and some of the ataractic drugs, but feels that psychotherapy is a necessary treatment need for most childhood schizophrenia.

The special psychotherapeutic needs of the schizophrenic child must be recognized: his overwhelming aggressive and libidinal impulses, his decreased capacity to withstand anxiety, and his need to be accepted at his own level (174). Although no statistical data, control groups, follow-up studies, or other standard methodology are presented in most of their published reports, there is a recurrent optimism among writers about the use of psychotherapy (44, 138, 176, 185) as a treatment method which can arrest or reverse the schizophrenic process in children. This type of belief is expressed in spite of the multiplicity of journal articles which present some highly divergent hypotheses as to what constitutes the core problem in childhood schizophrenia which has to be resolved [e.g., over-identification (138); ego-reinforcement (164); freeing of repressed, distorted libidinal impulses (185); as well as the significant differences in technique (all of which somehow still manage to be self-styled "psychoanalytically oriented psychotherapy") used to deal with the patient]. Starr (177) and Szurek & Berlin (185) consider the involvement of the parents in the therapeutic program as an essential aspect of the treatment whereas other clinicians (82) approach the problem as if separation of the child from the source of the devastating emotional deprivation which he experienced in the family constellation was a prime prerequisite for effective psychotherapy. It is obvious that a great need exists for clarification of the operations actually involved in what is categorized as psychotherapy, as well as for the appearance of new conceptual inventions which can provide an adequate interpretative framework for the processes whereby some of these behavioral changes can be understood.

ABNORMAL BEHAVIOR OF SENESENCE

The aging process is concurrent with the other fundamental life processes of development, maturation, and differentiation which are initiated by conception. Part-systems of the organism may involute and disappear early in childhood (e.g., the thymus), whereas the catabolic phase of other life functions does not gain ascendancy over the anabolic phase until much later in the life of the individual. However, it has been only comparatively recently that there has been recognition that the anabolic process (growth, differentiation, and maturation) may continue in some psychological functions well past the seventh decade of life in individuals under favorable environmental (cultural, physical, etc.) conditions. Alvarez (5), citing from a Japanese study, points out that the incidence of psychosis after the age of 55 turns noticeably downward there as contrasted with the situation in the United States and Europe. Perhaps the more clearly defined status of the aging person in the Japanese culture may be one of the factors contributing to this difference.

The second edition of *Mental Disorders in Later Life* edited by Kaplan (94) provides a much needed overview of the multiple aspects of abnormal behavior in later life. The various contributors not only attempt to review the research progress in their respective areas (physiological, psychological, psychiatric, sociological, nutritional, medical, etc.) during the 11 years since the first edition but also point up the issues which have research urgency. Learning theory, cognitive theory, personality theory, and motivational theory not only have to deal with the behavioral phenomena of senescence but may find "natural" experimental variation in function that may prove quite productive. The symposium on *Problems of the Mind in Later Life* (182) and the conference on *Aging and Retirement* (194a), together with the Kaplan book, provides a good orientation to anyone interested in these problems.

Kay *et al.* (98, 99, 100, 160), in a series of studies on the association of affective disorders arising in senescence with physical disease, found (a) that whether the first attack occurred after or before the age of 60, there was no greater incidence of cerebrovascular disease than in the normal population at this age; (b) that prognosis for improvement of the affective disorder and discharge from the hospital was good for both the pre- and post-60 group; and (c) that significant physical illness (either acute or chronic in character) was present in well over 50 per cent of patients suffering from affective disorders. The investigators conclude that the kind of cerebral degeneration associated with senile and arteriosclerotic psychoses is not contributory to causation of affective psychoses in later life, that while physical illness cannot be considered a specific etiological agent it is one important factor, one which physicians and surgeons attending the older patient with physical illness should be aware of, and that the physical illness played a part in breaking down previously effective psychological defenses.

Wilson (204) and Busse *et al.* (26) conclude from their independent studies of aged individuals showing marked senile changes that these changes reflect

a disease state which separates these individuals from the normal aging population, that genetic, biological, psychological, and sociological factors influence the types of behavioral pathology demonstrated, and that the process of senile deterioration is selective, i.e., the individual does not deteriorate equally in all traits.

Although the prognosis for discharge from hospitalization for patients with purely functional disorders is good (98), especially as compared with those older patients with acute organic-confusional states or dementia, there is a serious risk of suicidal attempts (134) among those older patients who have either psychotic or severe neurotic depressions. Even with treatment, Batchlor (15) reports that 12 per cent may be expected to complete the act within two years after the initial attempt.

Orme (136), using the Progressive Matrices, the Mill Hill Vocabulary Scale, and the Rorschach, confirmed previously reported clinical studies which emphasized that the affective disorders of later life were a distinct nosological entity from those psychoses with cerebral pathology. The Rorschach reactions were characteristic of depressive pathology in the affective disorders, whereas the senile dementia group reflected lack of insight, unawareness of inner conflict, and lack of integrated biological impulses.

The possibility of reversal of both physical and mental symptoms in cerebral arteriosclerosis with psychosis as contrasted with senile dementia (25) emphasizes the importance of differential diagnosis. Allen's clinical criteria are supplemented by Pichot's study (141) in which he found distinctive patterns of test performance (using the Progressive Matrices test of Penrose and Raven, and the Vocabulary test of Binois and Pichot) between these two groups. A high vocabulary level and low spatial reasoning were found with the senile demented, whereas the arteriosclerotic group showed a specific decline in vocabulary which Pichot correlates with the temporo-occipital localization of arteriosclerotic lesions (giving rise to a concept of "latent aphasia").

The generally more positive attitudes towards the capacity of the aged to respond to treatment is reflected in the favorable reports about the outcomes of various types of psychotherapy (individual, group, and even psychoanalytic) (71, 76, 119, 120) which are beginning to appear in the literature. Using the patient-doctor relationship as a basis for re-establishing the lost social contact, the development of creative activities for the patient, working through of the death fears (closely associated with changed body image and changes in body function), and a clear recognition by the therapist that the treatment relationship may have a long term character on an out-patient basis may help the elderly patient re-establish himself in the present. Frequent mention is made of the particular problems raised in the therapist when he has to deal with patients so much older than himself (120, 173).

Suter (181) found that 12 out of a group of 15 disoriented, confused, restless, untidy, and quarrelsome senile patients showed definite improvement (increased mental alertness, greater amenability toward management, increased self-care) with oral Metrazol therapy over a four to five week pe-

riod. The selective quieting effect of chlorpromazine with elderly psychotics who showed characteristic syndrome of chronic brain disorder is noted by Kurland (104) and Seager (168). They both observed a lowering of the hostile, destructive, agitated behavior of such dimension that many of these elderly patients could be returned from the hospital to their homes, or even avoid hospitalization entirely by chemotherapeutic control while in the home.

Reitman & Delgado-Fourzan (149) demonstrated that a modified type of ECT (atropine sulfate, intravenous pentothal sodium, and succinylcholine chloride) when given to 71 patients ranging in age from 60 to 83 years had no occurrence of any complications, even though many of the patients presented signs of severe physical disability. The problems treated included agitation, retardation, suspiciousness or self-depreciatory ideation, and especially depression. Either great improvement or complete remission of symptoms occurred in 70 per cent of the patients treated, while only 11.3 per cent were unchanged.

RECAPITULATION

The preceding review highlights two developments which may have considerable importance for the direction of psychobiological research and for the production of testable hypotheses about abnormal behavior reactions. The first is the experimental induction and control of "model psychoses" by the use of pharmacological and psychological agents. The availability of such a laboratory technology would seem to offer a potentially highly productive approach to controlled investigations of basic biopsychological processes associated with abnormal behavior reactions. The second important development is the impact which the ataractic drugs (which have had such a revolutionary influence upon the management and conception of the acutely disturbed psychiatric patient) may have upon theories of personality and psychopathology. The changes in psychomotor action, emotional reactivity, and intellectual clarity which have been reported by so many different observers in such a wide variety of situations and with so many different types of patients (acute, chronic, toxic, organic, etc.); the relative time and energy efficiency, on a mass basis, of a treatment procedure which produces both qualitative and quantitative alterations which are beyond the efficacy of any presently known method of short-long/deep-superficial psychotherapy; and the maintenance of the altered behavior either by very small doses of the ataractic drug (e.g., chlorpromazine, reserpine, promazine, etc.) or without any drugs, seem to have demonstrated unequivocally that there probably exist altered and attenuable psychophysiological states in the psychoses which may be precursors, as well as concomitants, of psychotic behavioral reactions. The refocussing of attention upon the biopsychological aspects of severe abnormal behavior reactions may necessitate a reassessment of many cherished psychological hypotheses about the etiology and "dynamics" of the psychoses.

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PSYCHOTHERAPY^{1,2}

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Publications on psychotherapy from the fields of psychology, psychiatry, and social work are the primary sources represented in this review. The majority of articles and books on this topic continue to be case reports, impressionistic evaluations of techniques and results with descriptively specified types of patients, and discussions of viewpoints. Priority here has been given to studies in which efforts have been made toward empirical explication of psychotherapeutic outcomes and processes. The many papers reporting on psychodynamics and psychopathology as revealed in therapy have been excluded since these subject matters are treated in other chapters. Many articles and a few books which could be very helpful to one wishing to practice or to do research on psychotherapy but are essentially restatements of existing material do not receive citation here nor do publications concerning professional, legal, and public relations problems.

The following brief delineation of the meaning attached to the term "psychotherapy" is offered in an effort to supply the reader with some information regarding the orientation of this reviewer. Psychotherapy is interpersonal relationships characterized by the following attributes. (a) One or more of the participants are expert in human relationships, implying effectiveness in altering of adjustment. (b) One or more participants have been designated as making unsatisfactory intrapersonal or interpersonal adjustments, or both. The designation may be by self or by others who are in a position to delineate the immediate life situation of this participant. (c) There is agreement by the participants, or those empowered to make compelling decisions regarding the participants, that the objective of the relationship is alteration of the unsatisfactory intrapersonal and interpersonal processes of the patient or client. It would be a mistake to forget that amelioration is one basic goal of psychotherapy and that the subjective state of the patient is under treatment as well as his more obvious overt characteristics.

This effort at definition suffers from ambiguities of meanings of terms, from overlap in meaning with other terms, and from deficiencies in knowledge. It serves to focus attention on the transactional character of the phenomena under consideration and the relativity of psychotherapy to interpersonal relationships in general. This relativity is especially apparent in the concept "adjustment." It is intended that the definition emphasize the obligation of the therapist to subordinate all other possible goals to that of

¹ The review covers the period from April, 1955 to April, 1956.

² The following abbreviation is used in this chapter MMPI (Minnesota Multiphasic Personality Inventory).

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promoting changes in the unsatisfactory aspects of the patient's intrapersonal and interpersonal processes.

The view of psychotherapy as interpersonal-transactional gives direction to selection of variables for study and guides interpretation of available data. The participants bring motivations, expectancies, and behavior patterns into their relationships, so that in part these relationships are seen as consequences of personality characteristic antecedents. Any particular, deliberately adopted role can be analyzed separately from the personality characteristics. For purposes of studying psychotherapy, this distinction is relevant since the deliberately adopted roles are techniques which are to be subjected to analysis. Psychotherapies are, also, relationships which have contexts which are determinants of these relationships. While clear distinctions between personality characteristics and environmental-social variables are often somewhat arbitrary, the distinction has heuristic value, currently, in that the contexts of psychotherapy begin to yield to analysis in terms of environmental-social variables. Reduction of such variables to interpersonal and intrapersonal variables will eventually be necessary. Thus, it is concluded that the analysis of psychotherapy should proceed, in part, in terms of patient variables, technique variables, therapist variables, environmental-social variables, and the interaction effects among them as related to therapy process and outcome.

Does psychotherapy result in changes? The phrasing of the question is deliberate, as the intention is to make the point that much of the research this year has been essentially untheoretical. In terms of long range planning, these projects are really pilot studies which have asked if there is any result of psychotherapy to be explained. Undoubtedly there are some clinicians who are offended that anyone should presume to doubt that psychotherapy does culminate in dramatic outcomes in desirable directions. Nevertheless, effects must be empirically demonstrated before the task of explanation and before the explanatory viewpoints can demand continued serious attention as psychology. The first section of this chapter deals with these outcome studies. If psychotherapeutic intervention does have demonstrable effects, study of the process and theory building is in order. The second section of the chapter is devoted to the process research of this year. The third section is a very selective account of certain trends represented in the discussion articles having to do with viewpoints and techniques of psychotherapy.

OUTCOME STUDIES

Even though logic would seem to demand that process be specified before outcome can be assessed most meaningfully, the majority of studies involving empirical procedures address themselves to the outcome problem. This is a healthy and understandable reaction to the challenges of skeptics and a natural first step in empirical validation of theories and methods of psychotherapy. These studies are reviewed with respect to the correlation of outcome with the following: patient variables, procedure variables, and environmental-social variables.

Patient variables.—First we interest ourselves in studies of patient char-

acteristics which are associated with favorable outcomes. Rosenbaum, Friedlander & Kaplan (69) separated 210 individual therapy patients in treatment with psychiatric residents into good, fair, and poor outcome groups on the basis of detailed check-list ratings by own therapists. Associated with greater improvement were better childhood environments, ability to develop interpersonal relationships, higher social station, better sexual adjustment, and more favorable financial status. Areas of improvement were primarily those of marital and work adjustment. The impression is that the result is more a maximizing of latent strengths than so-called basic changes in personality. If this is correct and is not what therapists have dreamed of, it is nevertheless a significant personal and social objective. Leary & Harvey (51) studied outcome of group therapy with a sample of general psychiatric clinic patients. Of interest in terms of patient variables are their comparisons of treated men with women. Men who are hostile or weak change more than women with the same characteristics, but if the prevailing symptoms are "bland and conventional," women change more. As usual, the number of cases is small and caution in interpretation must be exercised. Application of Barron's (8) Ego-Strength MMPI Scale to patients admitted to a hospital for treatment is reported by Wirt (96) as indicating validity of the scale for this type of population and treatment. A follow-up study of child guidance cases by Barbour & Beedell (6) shows that when children were matched for degree of maladjustment on admission, those who improved least or most slowly had histories of more birth, feeding, and toilet training problems. Interpretation of these data suggests that the chronicity of being symptomatic is probably more important than type of symptom (as traditionally defined) in predicting outcome. Treatment at earlier ages should then be more effective than at later ages, given the same age of symptom onset. Bolton (15) offers evidence giving some support to this idea. On the basis of these studies of patient characteristics, this reviewer draws the conclusion that improvement in therapy is related primarily to the level of previous adjustment, i.e., the extent to which appropriate roles were approximated. A corollary is that early treatment is more effective than later treatment. To give the impression that the reviewer would attach no importance to symptom content would be incorrect. But, pattern and content must be analyzed in terms of interpersonal significance before they become meaningful.

If it appears that very little has been done in relating patient variables to outcome of treatment, it is even more striking that the subject of interactions of patient variables with technique, therapist, and environmental-social variables is not represented. Although many impressionist analyses of therapy have been on this topic, these clinical hunches have not been translated into research projects. Certainly with limited treatment facilities, accurate prognosis is a matter of vital practical concern. Knowledge of the degree of modifiability of personality characteristics under varied, specified conditions would contribute not only to greater precision in psychotherapy but to the area of psychodynamics.

Up to this point, consideration has been given to studies in which patient

characteristics have been specified in terms of variables; a more traditional approach would be to think of patient characteristics in terms of diagnostic categories. Using this basis, improvement as a correlate or consequence of therapy has been claimed in the following patient categories: neurotics (5, 9, 10, 18, 36, 51, 53, 70, 76, 91), defectives (2, 68), reactions to disease or disability (19, 71), disturbed or problem children (6, 15, 56, 78), psychotics (5, 12, 17, 20, 24, 31, 58, 60), delinquents (50), psychosomatic or psychophysiological reactions or both (75, 84), normal and experimental subjects (33, 94), military cases (49), parents of behavior problem children (16), and prisoners (79). It is difficult to see any profit from these studies unless: (a) adequate controls are provided, (b) there is some convincing within category distribution on a severity or chronicity variable, (c) some means is devised for equating different diagnostic groups on severity of pathology permitting comparison of effectiveness of treatments for the categories, or (d) different techniques are applied to matched groups from the same category.

Studies of techniques.—Those reports which offer some definition of the therapy technique but only categorization of the patients are the subject of this section. Although projects in which alternative procedures are compared would be most relevant here, the section also includes studies contrasting therapy and control groups, and studies simply demonstrating change with therapy. Over-all, a wide variety of techniques are being subjected to evaluation, but there are few instances in which designs permit the kinds of conclusions which all would want to draw regarding prediction, course, and outcome.

An experimental study by Wiener (94) demonstrates that induced stress can be relieved more rapidly by counseling than by rest or talk. The study was well designed and suffers mainly from the intrinsic difficulty that laboratory stress of humans is mild in the hands of most investigators. More retrospective exploration with the subjects of their reactions during the various phases of the experiment would have been valuable. Fantasy, problem solving, and motor performances were assessed which shows commendable sensitivity to the need for representativeness in range of effects measured. The experimenter's effort to compare reassurance-interpretation and catharsis-reflection therapies was foiled because of the small amount of stress effect achieved. Among the studies of therapy with adults, that of Barron & Leary (9) reflects admirable attention to general design but a relative weakness of criterion measures. Using only the MMPI as the criterion, changes with and without psychotherapy were assessed. Psychoneurotics who had sought and been recommended for therapy were assigned to immediate and delayed treatment samples. Immediate treatment patients were assigned to interpersonal process centered group therapy or to brief, ego-oriented, psychoanalytically based individual therapy. The three groups did not differ in diagnosis nor prognosis, though because of judged need for immediate treatment patients were not assigned randomly to experimental and control conditions. All groups changed in the presumably desirable direction on several MMPI scales but only an increase in K for the individual treatment

group was significantly different from other groups. These authors suggest, most appropriately, that the decision to enter therapy and the effects of problem centered intake procedures should be assessed by comparisons with non-waiting list controls since the promise of forthcoming therapy should contribute very materially to stabilization and strengthening of coping processes. For example, Dymond (25) reports that delay patients who enter therapy only to leave very quickly showed strengthened defenses and denial of need for help. While delay control groups are desirable they are by no means sufficient since psychotherapy is a social institution and the decision to enter therapy is a transactional matter effecting adjustment. Taken together these studies by Wiener and by Barron & Leary support the conclusions that psychotherapy leads to certain limited modifications of behavior.

A fond wish frequent among friends of psychoanalysis is that psychoanalysts themselves would give more summary reports on outcomes of the treatment they offer. The multitude of individual case, technique, and theory oriented papers are undoubtedly very satisfying to the in-group, but leaves this sympathetic outsider with slim support for his faith and with insufficient definition of what the psychoanalysts themselves see as the consequences of so many couch hours. Schjelderup (76) has furnished a model which could be profitably followed by his fellows. He reports on 28 chronic neurotic patients who were psychoanalysed and contacted 8 to 14 years later. His cases are those seen before the German invasion of Norway resulted in his arrest. Typically they had been treated in other ways before they came to analysis presumably indicating guarded prognoses. Nine cases had lasting symptomatic cure and 13 showed substantial improvement of symptoms. There were favorable changes in interpersonal relationships for 25 cases and improvement of capacity for work and enjoyment of work in 22 cases. Favorable changes in sexual adjustment and in perception of reality are reported to have been frequent. The report reflects care and perspective on the part of the author and is very instructive as to what one psychoanalyst has undertaken and accomplished. A series of such reports would move much closer the time when realistic, well-designed studies of the effects of psychoanalysis as therapy could be initiated. There seems to be very little excuse for the absence of these data, and rationalizations will continue to be a very poor substitute.

Directive, educative, supportive therapy has been advocated rather actively and less apologetically this year. Some evidence of successful use of directive therapy with the more maladjusted of delinquent girls in a state institution is reported by Kosofsky (50). Immature, anxious, phobic patients who maintained close attachments to their mothers responded to a directive, educative, supportive procedure according to Tucker (91) who includes demonstration of the effects of the patients' hyperventilation as an essential component of treatment. Schindler (75) also proposes an educative-supportive therapy involving extensive use of audiovisual materials which explain psychophysiological reactions to general medical patients with "emotionally induced" illnesses. A one year follow-up showed the method far superior to

usual general practice procedures, but there is no comparison with conventional psychotherapy. The study has major methodological limitations, but despite the too glib categorization of patients and definitions of syndromes these results and the others cited in this paragraph may very well reflect a very important basic notion, i.e., education, reassurance, and supportive encouragement can reverse many decompensation phenomena. That these techniques can be misapplied is no justification for dismissing them. In somewhat the same general vein, Harris, Firestone & Wagner (36) report the expansion of the regular Marine Corps psychiatric screening interview into a brief psychotherapy session emphasizing anxiety relief through permissive and reassuring discussion of enuresis for an experimental group of recruits reporting that symptom. This very brief therapy resulted in adjustment which was better than that for two control groups. Serious consideration must be given the possibility that one major factor in psychotherapy is the extent to which the therapist can authoritatively restructure the field for the patient.

Psychotherapists have probably dealt primarily with cases which have proven refractory to formal or informal brief educative-supportive therapy and for whom the so-called depth approaches may be most appropriate. As psychotherapists deal more with early cases and as they are more involved in the training of people who meet such cases, attention to the directive, educative, supportive techniques becomes increasingly feasible. This is not to assert that these approaches when fully tested will be found to be superior to or as effective as those more psychodynamically oriented. It is rather a recognition that evidence is not now predominantly on either side.

Perhaps the most significant major development in the field of psychotherapy in recent years is group therapy. Though it has a fairly long history its general adoption is rather recent. Adaptations of group techniques are varied, imaginative, and vigorous. Zimet & Fine (98) found that group-centered technique, as compared with a lecture-group procedure, resulted in attitude changes toward self, other adults, children, and desirability of democratic principles. Working with general psychiatric clinic patients, Leary & Harvey (51) showed that treatment with group therapy resulted in significantly greater improvement than did waiting list placement when the criterion was a complex intrapersonal consistency measure. Dynamically oriented group therapy for prisoners in a special treatment center is reported by Showstack (79) to result in lower parolee return as compared with similar prisoners from other prisons not offering such treatment, and in a lower rate of within prison disciplinary infractions for treated as compared with untreated inmates. An outpatient parolee clinic continues treatment of paroled prisoners with apparent success in many cases, suggesting that therapy which bridges return to society is desirable.

Several studies report use of group therapy with schizophrenics. Rating chronic, severely disturbed, and agitated schizophrenics, Cowden, Zax & Sproles (24) found that changes in ward behavior were greatest for those who had group therapy plus reserpine, were equal for those who had group psy-

chotherapy plus placebo and for those who had reserpine only, while the control group showed no significant change. Another study of chronic schizophrenics by Funk *et al.* (31) compared a total push program emphasizing group therapy with a routine ward program. Ward behavior ratings favored the experimental procedure although there was no difference on psychomotor efficiency. A similar study by McCullough (58) resulted in improvement of insight, ward adjustment, and work adjustment. A two year study of habit training is reported on a small group of chronic schizophrenics [Bennett & Robertson (12)], who improved in attentiveness, alertness, and co-operation, and who did not deteriorate in personal appearance as did the controls. However, there was a progressive decrease in treatment effects over a two-year period, indicating a ceiling effect in this approach.

Group therapy with the chronically ill and handicapped continues to yield some encouraging results. Chronic peptic ulcer patients who had analytically oriented group psychotherapy [Stein, Steinhart & Cutler (84)] improved in ulcer symptomatology, character traits, and family relationships. Chafetz *et al.* (19) report a project with postencephalitic Parkinson's disease in which short-term group therapy was accompanied by reduction of denial and depression, clarification of medical aspects of the disability, and increased social participation. Another project used discussion group therapy, involving some interpretation of interpersonal reactions, with a small number of female adolescent and adult defectives over a 2½ year period. Patients who were depressive, passive, or paranoid profited most, but schizoid and aggressive cases seemed unable to stay in the program. Rate of return from parole was lower among patients who had group therapy than among somewhat comparable no-therapy cases [Astrachan (2)]. Ringelheim & Polatsek (68), utilizing group therapy with anxious adolescent and adult male defectives, obtained morale improvement and greater relaxation in interpersonal relationships.

Several tentative conclusions seem appropriate. The group treatment approach is flexible, and interest in it continues undiminished. It seems to be of special value in helping those who have become isolated to re-establish contact, e.g., invalidism and other secondary reactions can be reduced, transition out of institutions can be facilitated, and mental hospital adjustment can be improved. Socialization, better interpersonal relationships, and better work adjustment are the improvements which result most consistently from group therapy. Or is this impression an artifact of the kinds of measures and concepts employed by those who use this approach?

It appears that group therapy offers a way of reaching many patients who do not respond well to individual therapy and is an appropriate, practicable, and powerful tool in altering the environments of patients, especially children.

To what extent is the outcome of therapy attributable to restructuring of the environment rather than to changes in the patient, *per se*? Klumpner (49) raises this question rather forcefully in reporting on Army psychiatric patients. In attempting to assess the essential processes involved in improve-

ment of cases the author came to the conclusion that effects on commanding officers and others may have been of primary importance. Closely related are studies of family therapy. In a project on home subcoma insulin treatment Cohen & Clancy (20) draw the conclusion that family warmth toward the patient contributes to improvement. Here the brief psychotherapeutic aspect of treatment was directed primarily toward the family. Psychotherapy applied to both suicidal patients and their families is evaluated by Moss & Hamilton (60). Where the family treatment was judged to have had the most effect, the success with the patient was greatest. Parenthetically, in programs where treatment is given some of the family members it would be of great interest to assess other nontreated members of the families for the appearance of retrogressive phenomena since clinical reports indicate that complex reciprocal intrafamily effects are observed.

In general, it would seem that in many instances effective therapy must deal directly or indirectly with a situation rather than with the individual alone. The problem posed would seem to be: What interpersonal and intrapersonal fields are susceptible to restructuring, in what ways, and by what procedures? Modification of motivational-perceptual-cognitive-habit features of the individual (the more conventional concept of psychotherapy) is only one alternative within the above general conceptualization. There are some indications that the tendency to think either of psychotherapy of the patient or of intervention in the environment has given way to a broader concept in which these are seen as aspects in a comprehensive approach to treatment.

Over-all, a wide variety of techniques are being subjected to evaluation. There are few instances in which designs clearly support the conclusions. However, there are occasional projects which are well conceived and optimism that more and more evidence will become available seems justified. Briefly, the range of techniques studied is as follows: individual psychodynamically oriented therapy (9, 10, 60, 69, 70, 71), group therapy focused psychodynamically or interpersonally or both (2, 16, 17, 24, 31, 51, 58, 68, 79, 84), client-centered therapy (18, 92, 94, 98), directive therapy (50, 91), educative-supportive (19, 36, 75, 91), habit-training (12), perceptual isolation (5), play therapies (6, 33, 56, 78), psychoanalysis (76), and intervention in the environment (20, 49, 53, 60).

Therapist variables.—Viewing therapy as a very intimate relationship and recognizing a trend toward less restrictive therapist roles, the expectation arises that differential therapist characteristics would have systematic effects on process and outcome. A comparison of the expressed posttreatment moral values of adult patients who improved in individual therapy with pretreatment values of their therapists in the areas of sex, aggression, and authority indicated shifts in the direction of therapist attitudes. Values represented in the Allport-Vernon-Lindzey Scale did not change. Rosenthal (70) reports and interprets these data as disconfirmation of Mowrer's hypothesis that superego functions or values cannot be influenced by psychotherapy. Even though the relevance of these data to the classically defined

concept of superego can be questioned, the shift in important attitudes is impressive.

The many discussions on countertransference are relevant to this topic but are so lacking empirical explication that they are omitted here.

Environmental-social variables.—Imber, Nash & Stone (42), in offering confirmation of the relationship between social class and length of stay in therapy, have controlled therapist experience more adequately than has been done previously. Such studies would be more instructive if social class background of the psychotherapists and their sophistication regarding class mores were taken into account. It should be possible to rework several of the existing samples in terms of social class background of therapists. Group therapy for parents of behavior problem children sponsored by a school system is reported by Buchmuller & Gildea (16). Success was greatest in lower-middle and upper-lower districts where there is a greater degree of social uniformity, community spirit, and neighbourliness than in consolidated districts. The range of socioeconomic levels sampled appeared to be very limited, but the possibility that stronger neighborhood and community identification is a condition associated with greater success of treatment is fascinating. Is this a general principle which will help to explain the relationships between socioeconomic status and various phenomena of therapy?

Maas (56) and his collaborators studied sociocultural characteristics and attitudes of families who utilized clinic-type child guidance facilities. Managerial and professional groups were represented in caseloads more frequently, unskilled and semi-skilled classes as frequently, and white collar and skilled groups less frequently than expected from census data. Neither age of child, including the adolescent group, nor presenting complaint was related to outcome, but longer contacts were judged most effective. Self and school referrals were more frequently associated with favorable outcome than were court and hospital referrals. Upper socioeconomic cases were more often self and school referrals than were the lower socioeconomic. Retrospectively reported parental expectations indicated that when parents expected to be dealt with somewhat authoritatively and to have long contacts, outcome was more favorable.

It is certainly clear that, in general, patients' socioeconomic level is associated with therapy outcome. A further indicated step is identification of differential attitudes, expectancies, and therapy goals which differentiate among the social groups. Some of this has been done, e.g., patient beliefs about psychiatric practices. A second approach is to explore attitudinal correlates of success among groups of patients with homogeneous backgrounds. On the practical side, the latter approach may match technique to patient more quickly than the former. To reiterate, these environment-social correlates of success in therapy are only the preliminary findings which will lead to specification of interpersonal mechanisms or processes underlying these associations.

The environmental-social variables studied are as follows: social class (16, 42, 56), referral source (56), and pathogenic environment (69).

Methodological studies and outcomes.—Follow-up studies usually involve the difficulty that some proportion of the sample cannot be located. In their child guidance sample, Barbour & Beedell (6) found that difficulty of tracing of cases was related to sex, age, and referral source. Information of this kind alerts us to possible distorting effects in incomplete follow-up groups. Research lore such as this is one mark of maturation in a research area.

Barrabee, Barrabee & Finesinger (7) offer a normative social adjustment scale which could be helpful in specifying the before and after status of patients. More methodological contributions of this general kind would permit assessment of outcome by therapists whose responsibilities are primarily the giving of service. In addition, results reported at least in part in the form of such measures would offer a basis for relating different investigations.

The extensive use of self-ratings makes a study by Taylor (89) very pertinent. Repeated description of the self and self-ideal using a Q-sort method in the absence of therapy resulted in changes of the same kind but of smaller magnitude than those which occur with therapy. Increased self-consistency occurred so readily with repetitions that it is probably an inadequate therapy change criterion, but positiveness of self-concept and increased positive relationship between real and ideal self sorts seemed less susceptible to repetition effects.

The outcome studies can be summarized very briefly by indicating the range of effects which accompany treatment. Changes in the adjustive direction reported and given some degree of empirical support are as follows: interpersonal relationships (16, 19, 24, 31, 53, 56, 58, 69, 76, 84, 98), work adjustment (10, 49, 53, 58, 69), adjustment of children (6, 15, 56, 78), reduction of neurotic symptoms (5, 9, 18, 19, 36, 51, 68, 71, 91), strengthening of defenses (27), reduction of psychotic symptoms (5, 12, 17, 60), reduction of psychopathic symptoms (50, 79), reduction of psychosomatic or psychophysiological reactions or both (75, 84), decreased rate of reinstitutionalization (2, 48), and changes in self-concept and insight (5, 53, 70, 89, 98). These descriptive groupings are not mutually exclusive and tend to follow the terminology of the papers cited.

The empirical work of the year has been primarily in the form of outcome studies. Apparently psychotherapists are responding to the challenge to show that therapy does some good. Or, possibly, the motivations are primarily to learn more about the range of outcomes and to match techniques to patients more accurately. That therapy outcome will be found to be an exceedingly complex matter is indicated by the range of variables found to be associated with improvements.

PROCESS STUDIES

Precision in psychotherapy will be achieved through study of the ongoing process. Basically, outcome is some arbitrarily chosen point in process so that process and outcome variables should be rigorously related. Psychotherapy is one or a set of transactional relationships which span time, and the task of process analysis is to conceptualize and measure these phenom-

ena. There are but few commonly accepted reference points because little work has been done on the topic.

Technique variables.—In psychotherapy, it is assumed that the therapist adopts a set of ground rules which guide his behavior. Although the limiting case is probably never achieved the ideal therapist's contribution would be completely deliberate. This is not to say that therapy cannot be effective if done intuitively, nor that the therapist can be a disinterested administrator of doses of this and that. It is to say that the ideal therapist would provide whatever is essential to therapy (warmth, empathic emotional arousal, intellectual understanding, nonjudgmental understanding, rewards, and punishments, etc.) on the basis of reason and knowledge. The next step is obviously the investigation of the effects of defined techniques, when used with particular kinds of patients, within particular contexts.

The first requirement of any technique after there is some justification for believing it useful is the old one of reliability. The minimum conditions for reliability are those of definition of procedure and of a therapist who is personally qualified to follow the instructions. Clinical experience indicates that the latter is a very prominent problem in psychotherapy. Reliability of therapist role has received some attention from Matarazzo, Saslow, and their co-workers (57, 74) in their work on the interaction chronograph and from Moustakas & Schalock (61) in their studies of client-centered play therapy with children. Both studies described very limited patient contacts and relatively simple therapist roles. The over-all impression is that certain roles can be adopted consistently, although this matter so vital to reasoned psychotherapy must be explored much more thoroughly.

Research on process may have been deterred because many of the clinical concepts used are amorphous and do not translate easily into empirical procedures. The rocky road for this type of research is well illustrated by the important work of the Michigan group on interpretation. Having defined interpretation as any expressed therapist opinion of the nature of the patient's characteristics and depth of interpretation as the discrepancy between any such expressed opinion and the patient's awareness of that characteristic, Harway *et al.* (38) employed a Thurstone-type procedure to arrive at a depth of interpretation scale. Size of unit of material to be judged, method of presentation, and amount of the context of material available to judges were varied. Major conclusions were that depth ratings vary with unit size and context, but there is no difference between transcript and tape recording presentation methods. From the same project comes a paper by Raush *et al.* (65) reporting four studies of the dimensional characteristics of their depth of interpretation variable. Judges treat the variable as encompassing at least three dimensions, the primary one being depth of interpretation. The secondary dimensions depended on the judges and stimuli used. Those who can tolerate being sobered regarding the complexity of even gross quantification of such a technique as interpretation should give these papers careful attention.

Schmidl (77) makes a contribution to the problem of validation of psycho-

analytic interpretation. It is proposed that within Bernfeld's (13) five interpretive frames of reference each interpretation and its life history analogue must form a gestalt in the sense that both the organization and the elements of interpretation and analogue must match. This is a kind of internal consistency criterion which is not a sufficient condition for establishing validity but which is a step toward formalizing conditions for deciding whether an interpretation has been made. With some such definition of interpretation, consequences such as insight, behavioral change, and physiological reactions could be studied with some confidence that the process under study had been made explicit.

In play therapy there is opportunity to relate technique, objectified in toys and activities provided, to process. Popularity, communication value, and potential for stimulation of fantasy of toys commonly used in play rooms were assessed for a clinic sample by Beiser (11). Doll families, soldiers, guns, clay, and paper and crayons were the toys which proved high on these characteristics. Further systematic exploration of the stimulus values of play materials would provide baselines against which to interpret play therapy activities. Gump & Sutton-Smith (33) take the view that responses are determined by activity. Group swimming was compared with group crafts to determine the frequency of six categories of interaction and the amount of interaction which involved the counselor. Swimming evoked more interactions and involved the counselor less. Sharing was the predominant category in both activities, but helping was second in crafts, while asserting-attacking was second in swimming. The authors' suggestion of further analyses of other activities and effects on different types of participants has much to recommend it in terms of developing selective treatments.

In a study of actual process, Moustakas & Schalock (61) found that the therapist behavior which they called "interpretation" (probably to most observers "reflection") was most frequently followed in play sessions with children by the patient response of acceptance.

An intriguing experimental paper on interpersonal process is Verplanck's (93) study of the control of verbal behavior by approval or paraphrasing; so-called operant conditioning. In essence, the subject, who does not know he is a subject, is led to give more and more statements of opinion when the experimenter approves or paraphrases spontaneous statements of opinion by the subject. While there are several experimental shortcomings which are noted by the author, it is interesting to speculate from these results. One line of thought would consider psychotherapy as a complex and not always efficient conditioning process in which the patient learns to talk differently and little else. Another line of speculation not tested by the experiment is that while there are orderly changes in verbal behavior, the essential correlates are changes in the patient's perception of himself as a result of the fact that someone else spends time with him, takes what he says seriously, and shows him respect. Or, one might suppose that reorganization of verbal behavior would be accompanied by substantial modifications in other behaviors, especially those which are talked about. The methodology holds promise for in

vestigations of psychotherapy-like situations and as one method of analyzing psychotherapeutic process.

In summary, technique as related to psychotherapeutic process has been studied as follows: effects of personal appearance of patients before supervisors or clinic groups (52), combination of group and individual therapy (14), methods of measuring interpretation and consequences of interpretation (38, 61, 65, 77), effects of physical restraint with children (88), one person acting as therapist for several family members (63, 83, 88), and stimulus value of toys and activities (11, 33). These represent only a very small sample of the technique variables suggested in the clinical literature.

Technique and environmental-social variables.—In an article in which the conclusions seem to have been reached more impressionistically than empirically, Maas (55) reports that patients from "respect-solidarity" families (lower class "Old World milieus" such as Irish and Italian) make the transition into group therapy best if patient-patient relationships are fostered. Middle class "nonethnic American and Jewish backgrounds" dispose patients to fare better if patient-therapist relationships are given emphasis. Methodologically, the study is far from satisfactory, but it illustrates a conceptual approach which should be followed up by adequately designed research. There is the very provocative suggestion that psychotherapeutic procedures appropriate for lower class and other resistant groups can be devised by reference to the mores of the subculture.

Patient and therapist variables.—The more conventional concepts of therapy would direct the attention of the investigator to the characteristics of the patient, in the attempt to understand the process. This results in part from the definition of rather inflexible therapist roles and of psychodynamic concepts which emphasize intrapersonal conflict. If there is a trend toward more flexible therapist roles and toward more emphasis on viewing maladaptive phenomena as interpersonal, then process analysis will focus increasingly on the interaction of therapist and patient. In an ingenious analysis, Coleman, Greenblatt & Solomon (21) combine physiological measures and observer ratings to study the interaction of patient and therapist. Illustrating the method on a single case they found that heart rate of the patient was highest for periods when observers rated the patient as anxious, lowest for depression, and intermediate for hostility. During the 6 hr. of therapy experienced as most disturbing by the therapist, the correlation between therapist and patient heart rates was lower than during the 6 hr. when the therapist felt most comfortable. The authors of the study interpret these last findings as a possible objectification of degrees of empathy.

Turning to the question of contributions of the therapist, a paper by Strupp (85) presents some correlates of personal therapy of therapists. Therapists with and without personal therapy were asked to say how they would respond to a series of excerpts from therapy. Analysed therapists indicated that they would be less silent in general and would tend to respond to transference phenomena with interpretation and structuring as well as with silence. Both groups were disposed to respond to suicide threats with reassurance.

This type of study may generate hypotheses, but some skepticism regarding generalization to therapy sessions seems justifiable inasmuch as context does affect interpretation and probably other classes of therapist activity. The method could be very useful in having therapists generate statements of psychotherapeutic viewpoints, however.

Parloff (62), approaching the study of process, used the global concept of "excellence" of therapist performance. He uses actual therapy sessions experimentally so that therapists served groups for two weeks and were then replaced by other therapists for two weeks. Each therapist was rated on effectiveness in general social relationships by a second panel of judges, and the two sets of ratings were positively correlated. This type of study in which one set of judgments of therapists is related to ratings of the same therapists by other judges seems inviting but may be only a kind of complex popularity assessment. In the same study, it was found that a therapist relates best to those patients who most closely approximate his "Ideal Patient." It can be anticipated that some clinicians would object to substitution of therapist for "experimental purposes," and some evaluation of effects of this kind of procedure should be made.

A process study of client-centered therapy with children was conducted by Moustakas & Schalock (61) who analyzed interactions in play sessions with two four-year old groups. The emotional problem group had more boys so the finding that they were more nonattentive and expressed more hostility may be a sex difference. In any case, it is clear that the relationship develops differently even though the therapists' roles are very similar with both groups.

Many investigators will probably want to use some form of content analysis in process studies. Auld & Murray (3) have provided a useful review of studies of psychotherapy in which this method has been used.

The small number of process studies is not surprising considering the complexities and time involved. This type of research is of particular relevance in finding out what alternative psychotherapeutic procedures are used and by what therapists, with what patients, from what environments, and with what outcomes. Individual case studies, series of cases, viewpoint papers, and descriptions of techniques can serve a fundamentally useful purpose only if they culminate in process investigations.

VIEWPOINTS AND TECHNIQUES

A substantial proportion of the papers and books on psychotherapy are discussion of viewpoints and techniques. While often based on and illustrated with case material, viewpoints and techniques are related to data so impressionistically as to fall outside any definition of empirical investigation. These publications are certainly not disparaged. In fact, if the task at hand were to teach someone to be a psychotherapist, this literature would be far more useful than the research articles. The reviewer has chosen to reflect rather than to summarize the developments and trends in this type of literature through examples which represent a very small proportion of these publications.

Flexibility or even eclecticism.—There is a feeling of developing eclecticism

in the air. Symonds (86) amalgamates Freudian, neo-Freudian, and phenomenological concepts in presenting his psychodynamic view of psychoneuroses. He discusses psychotherapy under headings of definition, process, indications for, contraindications for, and goals. Abreaction, which is said to be the principal dynamic factor in therapy, occurs only after the therapist begins to be seen as nonthreatening and understanding. After abreaction, there is a changed perception of self which is hastened and stabilized by interpretation and insight. Symonds' level of discussion is introductory but sophisticated, and many experienced therapists will find themselves reminded of important considerations often ignored in the press of an overfull schedule. That procedures, goals, and concepts can become ritualized and therapy of the patient made secondary is emphasized by Reider (67). Though Henderson (40) argues for the superiority of the Jungian view of transference, he discusses it and the Freudian formulation as somewhat supplementary. There are many evidences of flexibility as psychotherapy is applied in diverse settings and with types of cases where it would not previously have been deemed feasible. In psychotherapy with psychotics, Ellis (26) finds variations of methods depending on personalities of patients. A review of the 1955 group therapy literature led Hallowitz *et al.* (34) to point to the diversification of purposes, settings, and procedures in group therapy. On the basis of the above article the present reviewer experienced intensification of an impression of a paucity of empirical studies.

The same atmosphere of renewed interest in seeking, debating, and trying variations of methods prevails in the area of child therapy. Woltmann (97) in a review of hypotheses regarding play therapy found a wide range of assumptions which seemed to share agreement that play is intrinsically integrative and a natural mode of communication. The reviewer would conclude that it is play in the presence of an understanding adult which is integrative, not that play is intrinsically integrative. A play interview technique which is closely related to release therapy is discussed by Conn (23). The child is furnished with deliberately selected dolls for whom he speaks and for whom the therapist occasionally furnishes dialogue at crucial points. Hambidge (35) reports on amplification of release therapy for cases where typical interpersonal problems have arisen. Structured and free play are interposed in a flexible way. Special psychotherapeutic programs and procedures beyond those of the clinic or mental hospital but stemming from them are exemplified in a discussion by Jensen (44) of care of hospitalized children. A panel of authorities, in considering treatment of adolescents, discussed parent participation and other environmental restructuring (46). Rejecting the usual child guidance approach involving several therapists per family, Starr (83) argues that evidence justifies viewing the mother-child relationship as crucial in personality development and that treatments are maximally complementary when both mother and child are seen by a single therapist. A somewhat more radical innovation is proposed by Pechey (63) who tries to deal directly with the family relationships which are assumed to be blocking natural growth processes in maladjusted children. Parent-child play sessions are observed directly and are then discussed by therapist and mother. Variable in its de-

tails, the method is said to be adaptable to many problems with the compelling advantages that remaking of relationships remains in the hands of parents and continuity with total life situation is high.

Revision and reformulation of psychoanalytic theory and technique continues. The disposition to make basic changes seems to be gaining momentum and promises to result in rather drastic changes [e.g., Grinker (32) and Fairbairn (28)]. Although efforts are being made to salvage libido theory as an integral and important part of these reformulations, the core concepts are interpersonal-transactional. This should not suggest that Freudianism is being deserted, but it is to say that there is a lively and encouraging trend toward going beyond Freud.

Relationship, transference, and countertransference.—On the basis of more than 400 articles, Ellis (27) concludes that compared with 10 to 20 years ago there are numerous and contradictory procedures advocated, that orthodoxy is waning, that there is more attention to selection and stabilization of patient goals and values, and that there is increased emphasis on relationship in therapy with a more collaborative, active patient-therapist interaction.

This conclusion applies to the reports of this year. Of particular interest are the indications in the psychoanalytic literature of a continuing flirtation with what are becoming radical revisions of Freudianism. Brief mention of this trend was made above. The psychoanalytic concepts of psychotherapeutic process, transference, and countertransference illustrate the direction of the revision. Tower (90) points out that there are, in fact, many analyst responses in the treatment situation which are not countertransference, e.g., intellectual comprehension, ego-adaptive responses, intuition, and rapport. It is the last type of response which is of particular interest here since it suggests an affective interaction which carries the therapy. It is emphasized by Tower that countertransference means the transference of the analyst in the treatment situation and nothing else. Several motives which operate in and are satisfied by the practice of therapy are discussed by Szasz (87), who indicates that the therapist's acknowledgment of these satisfactions is desirable, perhaps even necessary. These are not unusual examples of the growing awareness that psychoanalysis is an interactive relationship in which gratifications go in both directions. Viewing the therapy situation in terms of field-theory, the problem of countertransference can be, according to Colm (22), realistically reinterpreted. There need not be a choice between countertransference and identification, because the fundamental fact of interaction makes countertransference necessary. Parataxic countertransference reactions are to be avoided by achieving openness within oneself which permits openness and sharing of a relationship with the patient. Reactions of the therapist which will help the patient understand his own effect on others and which reveal interest and concern on the part of the therapist are appropriate. (The difficulty which arises in terminology is well illustrated by the different meanings of countertransference for Tower and Colm.) The issue of openness of the therapist is seen by Colm as being particularly pertinent in therapy with patients during latency and adolescence in whom suspiciousness and defensiveness are especially powerful and may prevent patients from revealing

themselves. Whereas transference has seemed to mean that the patient distorts in particular ways, the term has been used to mean implicitly that the patient must receive affection from the therapist, i.e., must be dependent on the therapist, and that the patient gives affection to the therapist. These latter meanings are better designated by the use of the concept of relationship. The reviewer reaches the general conclusion that some psychoanalysts are redefining psychoanalysis, as therapy, as a process fundamentally attributable to relationship, and psychoanalytic personality theory is being redefined in terms of interpersonal concepts.

Acting out.—Silverberg (80) shows that insight may be instrumental in promoting acting out because insight produced in therapy may be interpreted as restrictive or disciplinary. Thus, even insight is not an end-point internal in the patient but is a restructuring of relationships, particularly with the therapist. According to a formulation by Ruesch (73) the problem of therapy is essentially that of helping the patient to use the various forms of codification in an optimal way so that the feeling of satisfactory communication can be achieved. Deficiencies in mastery of nonverbal and verbal codifications or in synchronization of these modes may occur. Restriction of the therapist's role to the use of verbal language and particularly the restriction from action language limits the extent to which effects can be obtained within the therapeutic relationships so that acting out is inescapable in many cases where use of action language is necessary to establish the full repertoire of communication techniques and their integration. Moreno (59) offers a critique of the Freudian couch, free-association, interpretation technique, making the point that the system fails to provide adequately for psychomotor and action events as therapeutic media. This is an important consideration and promises to receive continuing attention. Combination of group and individual therapy in order to reduce acting out in the environment and dependency on the therapist is advocated by Beukenkamp (14).

Material which could be translated into acting out scales is provided by Slavson (81) and by Alexander & Pope (1). Acting out is viewed as evidence of negative transference with specifiable antecedents, characteristic forms, and predictable outcomes. Slavson suggests that acting out is to be handled in terms of the positive transference, the group process, and the feelings of the therapist himself. Acting out of children, in Slavson's view, emerges more appropriately than in adults because of their incomplete ego development and is an essential element in child therapy as catharsis is accomplished in this way. It seems plausible that adult patients who are impulsive might require a treatment situation like child therapy in respect to acting out opportunities.

Child therapy.—Klein (48) provides a welcome statement of her current views on technique and theory of psychoanalytic play technique. The focus of the method remains on anxieties and defenses, particularly as revealed in the transference. Early, frequent, and deep interpretations are emphasized, and toys are chosen for their value in facilitating fantasy. One concludes from the case material that it is interpersonal fantasies which are the important grist for the therapeutic mill.

Within a contemporary psychoanalytical theoretical framework, consideration is given by Hellersberg (39) to treatment of neurotic children. Play is thought to permit renewal and expansion of contact with the world, being the medium in which needs and capacities can be experienced and integrated. These are the raw materials for definition of ego boundaries and ego strength. The first requirements for therapy are freedom from adult interference and anxiety and suitable materials which can be used in expression. In particular, the child needs to use his sensory-tactile and muscular-motor capacities to recapture primitive satisfactions. Constructive and projective play become the media for the understanding of relationships to the inanimate and animate environment. Identifications are also explored in this play. The nurturant and protective attitudes of the therapist permit these developments.

Solomon (82) proposes that ego development which can be the basis for progressively more mature adjustment can be re-established in play therapy by (a) deriving the actual life experiences from the elaboration of fantasy, (b) converting perceptual to conceptual thinking, (c) mastery of conflict through the therapeutic relationship, (d) development of time perspective, and (e) mastery of the current environmental situation. According to Axline (4), the common therapeutic element in play therapies is the opportunity for effective experience and self-exploration within the protection of a warm respectful relationship. Responsible freedom to express capacities is developed.

The common elements of these viewpoints may be an illusion created by the words used. One is impressed, however, by the emphasis on freeing the child to use his capacities and to experience gratifications of several kinds. There is the feeling that there is a strong family resemblance between these statements about the tasks of therapy and the renaissance of positive hedonism in motivation theory in general.

Group therapy.—There is a high level of interest in and discussion of group therapy. Reviews by Hallowitz *et al.* (34), Fraser (29), and Listwan (54) represent much of the current material. Since these reviews and that of Harris (37) last year cover discussions of the topic rather well, nothing will be added here.

Pharmacological adjuvants.—In order to lay any claim to timeliness, a review on our topic must make some mention of "tranquillizing drugs." There are of course numerous impressionistic, favorable evaluations, which assert that patients are made more accessible to individual, group, and hospitalization treatment. Specific utilization of pharmacological agents in connection with psychotherapy is probably best illustrated by Rothman & Sward (72). Thiopental sodium and methamphetamine were combined specifically to increase spontaneity, rapport, interpersonal communication, and feelings of well-being in patients who had not responded to psychoanalytic treatment. The point is that the enthusiasts for tranquillizers could contribute much more if they would follow this example and try to test specific ways in which these drugs effect the phenomena of therapy. In general, the literature suggests that tranquillizers are especially useful adjuvants to treatment of psychotics.

Treatment of psychotics.—Szurek & Berlin (88), using a procedure based

explicitly on a psychogenic theory, offer a paper on therapy with schizophrenic children and their parents. They advocate use of physical restraint sufficient to prevent disintegrative consequences in treatment. If such restraint is applied without ambivalence, the child typically shows relief and indulges in some self-gratification which is accepted by the therapist with the result that the first phase of treatment, identification with the therapist in regard to restraint of destructive behavior and achieving sensual gratification, is accomplished. If one person is therapist for all family members, there is less identification with any family member, a clearer understanding of each member in his environment, and rivalry among family members can be handled therapeutically. Conviction that therapist's personality is especially important in treatment of schizophrenics is widespread [e.g., Jackson (43)]. Without disputing possible hereditary predisposing factors, Hill (41) presents a discussion of schizophrenia in terms of personality development as an interpersonal process. Self-understanding and nondefensiveness are seen as the optimal therapist characteristics, and usefulness in helping the patient to discern and relate to reality is the characteristic of the interpersonal relationship which is therapeutically potent for schizophrenics. Hill believes that these patients have a profound appreciation for material which is unconscious in neurotics, thus posing a different therapeutic problem. The discussion of therapy is brief and could have been more profitable if extended. In the opinion of Winnicott (95), sensitive therapists will be profoundly affected by psychotics they treat. A continued trend in treatment of psychotics is noted in the use of what has been referred to as total push, hospital treatment, and the therapeutic community approaches (30, 45, 64, 66). All of these involve a renewed interest in what might be called special environment therapy. That those engaged in psychotherapy give increasing attention to the total setting of which treatment is a part is strongly implied by these approaches.

OVERVIEW

There can be little doubt that the number of researches on psychotherapy is increasing. With a continuation of the trend of the past few years we shall soon have generally adequate outcome research on diverse samples of patients and techniques. One necessary feature of outcome studies which has not been mentioned here which deserves emphasis is that of follow-up assessments. It is to be hoped that adequately designed projects will be continued as long-term follow-up studies. Assuming that outcome research will establish that psychotherapeutic techniques do result in significant and desirable effects, there will be added impetus to process research in order to increase efficiency and explicate how the effects occur. As of now, the amount of process research being reported is small, but over the past few years techniques and concepts useful in this type of study have been appearing consistently. There is a sense of increasing sureness as to how to go about process analyses, there is some additiveness to methodological contributions, and there are continuing efforts to theorize effectively about psychotherapy. The traditional reliance on repression and insight type of formulation is not being abandoned but is sharing the arena with many robust companions, e.g.,

social learning theories and self-theories. Uncovering as the primary technique has taken its place with relearning on the basis relationship, support and strengthening of defenses, and a renewed interest in educative-informative techniques. All of this is intended to convey an impression of progress and hope.

Some definite tempering of this optimism is now in order. Clearly it is an optimism based in significant part on direction rather than point of progress. The range of patient, therapist, technique, and environmental-social variables represented in research should be extended. Outcome criteria could be substantially improved, but this will involve resisting the lure of convenience by selecting measures which are intrinsically powerful, e.g., measures based on how the person behaves and relates to others in his life environment and how he assesses his state of happiness. Therapy with families, including therapy with children who are having problems, could be assessed most readily in this way, since school, peer, and sibling relationships, and achievement can all be brought under study relatively easily. Representativeness (variety) in outcome measures is essential. It is still uncommon to find significance tests applied to data and to find control groups. The effort must be made to recruit experienced, expert therapists for research cases so that maximum effectiveness can be reflected in results. The continuation of efforts to relate personality theories and general behavior theory to psychotherapy is indicated, as this has not been accomplished.

During the year covered by this review, there were few books which represented new formulations, that by Kelly (47) being the strongest candidate. The year was one in which psychotherapy research had not yet come of age methodologically but showed real promise of doing so in the immediate future. To this reviewer, it was also a year in which a substantial number of psychoanalysts, general dynamically oriented psychotherapists, client-centered therapists, play therapists, and directive therapists showed interest in adopting more flexible, exploratory approaches in viewpoint and techniques.

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THE THEORY AND TECHNIQUE OF ASSESSMENT^{1,2}

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INTRODUCTION

The year 1955 may have been an emergent one for psychology. Certainly it has produced a literature which is different. Dynamic understandings now enhance the value of static descriptions and the trait philosophy of behavior is no longer restrictive as it guides the thoughts of those who study the individual.

There is a refreshing trend away from gross empirical validations which required that tests predict the diagnostic decisions of psychiatrists or psychologists. Instead there seems to be an emphasis on the conceptual validity of the procedures employed in assessment. Studies of projective instruments, at one time vulnerable because of an uncritical acceptance of implicit concepts, seem now to be characterized by an almost obsessive concern with the validity of the constructs which are part of the projective approach. One can discern a lively interest in the broad behavioral significance of all responses.

Psychologists have become so psychological in their inquiries that they have begun to scrutinize their own reports and to analyze the efficacy of their communications. Such concern quickly leads to the problem of defining the purposes for which a report is prepared and anticipating the knowledge and the motives of the man who must use the report. This takes us to the problem of criteria, and it is here that the literature is weak. In both their conceptualizations and their instrumentations psychologists are prediction oriented. There is little evidence in the present literature that the bold ingenuity which characterizes the psychologist's analysis of the process of prediction characterizes his interest in the purposes of prediction. This is paradoxical. While the logical and empirical basis for well-known testing instruments is subject to scrutiny by innumerable investigators, there is no comparable consistency of interest with respect to common criteria. Perhaps specific criteria will never be subject to the kind of scrutiny that is now directed toward tests. In the study of assessment our emphasis seems rapidly to be expanding toward a concern for behavioral constructs and logical models. In this kind of a development the great concern may be with the validity of our theory of behavior and with the efficacy of the logical models whereby we apply our observations to our purposes.

Many of the most imaginative and well-designed studies are contributed by relatively new names and do not acknowledge the support of foundations

¹ The survey of the literature pertaining to this review was completed in April, 1956.

² The following abbreviations have been used in this chapter: TAT (Thematic Apperception Test; WISC (Wechsler Intelligence Scales for Children).

or research institutions. If this means that a large quantity of this new research is being done spontaneously by young psychologists working with the personal enthusiasm which unsupported research implies, we have found assurance for the future.

Interest in assessment may be primarily diagnostic with a concern for possible further deterioration or for possible benefit from therapeutic influences. Contrastingly, the object of interest may be the individual's strength, either in meeting relatively short-term demands or in fulfilling lifelong expectations. Regardless of our goal, it is becoming increasingly obvious that virtually all of the conceptual achievements of psychology and all of the refinements of experimental design may be employed in this area of inquiry (1). Assessment, a relatively unusual word in the psychological literature of 15 years ago, now refers to a rich new field where psychology may grow in both professional and scientific stature as it applies its accumulated knowledge and skills of inquiry (2). The resourceful use of psychology has not always been characteristic of our studies in the field of assessment and diagnosis. As recently as 1954, after a survey of then current literature, this reviewer was obliged to comment on the limited scope of most clinical investigations (3).

THE CONDITIONAL SIGNIFICANCE OF TEST BEHAVIOR

The literature provides a diversity of reports illustrating factors and circumstances which require a qualified use of test results. For example, Mindess' discussion of psychoanalytic symbolism and the Rorschach test (4) asks for a qualified use of responses to projective test material. He advises the reader that the symbolic interpretation of the content of Rorschach responses is logically defensible only if one considers the subject's conscious motivation. His report, like Davids' experiments (5), is related to Allport's position (6) that projective techniques tell you no more about the man than he can tell you about himself if he is not handicapped by a neurotic condition. The importance of the individual's motivation receives further emphasis from Levy (7) who shows that the perception of human movement in ambiguous figures is greatly influenced by the preliminary instruction to look for human figures. Starer's (8) report suggests that some of the motivating circumstances qualifying the results of assessment procedures may be relatively long-lived under stable social circumstances.

Alper, Blane & Abrams (9) described lower class children who used finger paints more freely and spontaneously than middle class children. This result was predicted from the fact that toilet training is restrictive and frustrating to the middle class children. The possibility of cultural factors qualifying the meaning of responses to projective tests is carried further in a report by McCary (10) who shows that both racial and geographical factors seem to be correlated with differences in the Rosenzweig test. Northern persons were more extrapunitive than Southern persons, and Northern Negroes were more extrapunitive than Northern whites. McArthur (11) has described

possible relationships between class status and responses on such devices as the Rorschach.

Whether projective instruments, such as the TAT, express temporary characteristics of the individual or only enduring characteristics is examined by Lindzey & Herman (12). They succeeded in predicting the direction in which the TAT productions were changed in consequence of a frustrating situation; thus these authors, like McClelland (13) and others, have shown that the TAT is responsive to temporary circumstances. This raises the general question whether the instrument may be oversensitive to current stresses and thereby may obscure underlying permanent characteristics. (This, of course, assumes that there are underlying characteristics which are different in kind from the temporary reactions. Just as therapists have come to recognize that therapy does not begin or wait according to the therapist's will, students of assessment may come to recognize that the behavior manifested by a subject is not temporary or permanent according to the will or intention of the examiner.) The evidence that reactions to projective material may vary according to the circumstances of the person is quite interesting in its diversity. For example, Meyer, Brown & Levine (14) show that just prior to surgery patients show a temporary deterioration of performance on the Draw A Person Test. This suggests that some of the deterioration found in the productions of mental hospital patients may be attributable in part to an anxiety about their circumstances.

Bash (15) reports a study in which he asks his subjects to report what they see in a series of 200 successive brief presentations of Rorschach Card 9. Through these 200 trials there was a shift from one *erlebnyinstypus* to another. This suggests that some of the reactions to projective tests may not be lifelong and characterological, but somewhat intermittent and perhaps in part a reaction to temporary satiation or excitement. Marchand (16) showed that the IQ of mental defectives may rise after employment, and Taylor (17) showed changes in patients who repeatedly expressed their self-concepts by means of *Q*-sorts.

Some reports are directly relevant to the examination situation *per se*. Using white subjects, Rankin & Campbell (18) showed a much greater galvanic skin response to a Negro examiner than to a white examiner. If such a seemingly minor factor can produce a change, other things more directly relevant to the examinee's status should receive serious consideration. Landfield (19) reports that in predicting their own behavior subjects are less competent in the presence of threatening persons than in the presence of non-threatening persons. This obviously suggests that self-evaluations should be sought under standard circumstances. Some possible influences of the examiner have been explored in the experimental tradition by Wickes (20). He found that if the examiner interpolates either verbal ("good," "mm," "alright") or nonverbal (smiling, nodding the head, or leaning forward) rewards for movement responses, the probability of movement responses to succeeding cards is increased. This effect should be carefully cross-validated;

if it is generally applicable to projective testing situations, verbal responses of the examiner must be standardized.

THE ASSESSOR AND HIS USE OF INFORMATION

The manner in which the assessor uses his information to arrive at a comprehensive assessment has been the subject of repeated comments in the literature. Among clinicians there is probably great diversity in this aspect of their procedures. Although by definition those who study the individual are inclined to be ideographic in their emphasis, they are likely also to acknowledge that a reductionistic analysis of their methods will lead to the nomothetic point of view. This is not true of all clinicians, however, and perhaps it is fortunate that articles like Falk's (21) continue to appear. Some writers have felt that a clinical science cannot develop until the clinician is better understood in his clinical assessment role. The unique, quasi-private, unanalyzed, if not unanalyzable, nature of the clinician's judgment leads Hammond (22) to remark that a clinician may be more of an instrument himself than a reader of instruments and in this sense should be a proper subject of inquiry. He notes that many clinical instruments or tests are in effect devices to reproduce the kind of judgment that the clinician renders. This seems to Hammond to be a major difficulty because it proposes that a public instrument be employed for the prediction of a private criterion and that necessary limitations obtain in this situation because the nature and the limitations of the criterion itself are not understood. Hammond also draws to the reader's attention the phenomenon of vicarious functioning, pointing out that the organism may substitute one form of behavior for another in order to achieve its goal. This phenomenon of vicarious functioning is seen to lie at the heart of the private, quasi-rational nature of the clinical decision. This character of human behavior has been the subject of study by Wittenborn, who in a preliminary report employs a simple correlational model to approximate some of the clinician's inferential process in arriving at a judgment of the meaning of an individual's behavior (23). A study by Todd (24) indicates that the use of a probabilistic multiple correlation model can be employed to provide an improved judgment of the clinician's estimate of intelligence from the Rorschach.

Some errors in clinician's judgments have their origin in a disregard for the varying probability with which certain types of conditions occur in different samples. Many clinical signs and tests were originally based on comparisons where half of the subjects were normal and half were suffering from pathology. The differences observed for these subjects were later used as a guide in a screening or in a diagnostic situation. Such a use, however, carries the implication that in the practical situation the probability of the individual's being rejectable would be the same as it was in the original comparison. Actually, however, in the population to which the screening device is applied the probability of the pathological or the rejectable condition may be only 5 or 10 per cent. In such situations it is often found that the cut-off point

(or other discriminating distinctions) based on comparisons from the selected original samples is grossly inappropriate and results in a very large number of false positives. Such difficulties are analyzed by Meehl & Rosen (25), who refer to well-known diagnostic or screening procedures. Their discussion can be related to one provided by Patterson (26).

The hazards of accepting a stereotype based on one kind of population and naively applying it to a different kind of population are discussed by Gage & Cronbach (27). Although these investigators are concerned primarily with social perceptions in judging others, their discussions have implications for the clinician. Cronbach (28) offers a kind of semimathematical analysis of some of the factors in social perception and indicates sources of error.

A perennial part of the clinician's procedure is the use of difference scores or profile patterns. Whether one refers to the use of simple differences and ratios or to complex profiles, the lack of a suitable rationale is perplexingly absent. The use of such differences involves an arbitrary weighting of the component variables and is uneconomical (if more than three variables are involved) because the number of differences is much greater than the number of variables from which the differences may be generated (29). Lykken (30) very sensibly assumes that the value of profiles, if they have any advantage at all, accrues from the nonlinear nature of the interrelationships among the different variables.

The logical operations which are involved in the assessment of the individual are considered also from the standpoint of the psychology of the assessor. One of the most valuable accounts has been provided by Hathaway (31) who offers four useful descriptive classifications of some of the inferential judgments of clinicians. In the first class the clinician's judgment accrues from his ability to estimate the correlation between items of behavior and relevant criteria. In the second class the judge recognizes bits of behavior as belonging to some familiar larger class of behavior. A third class of judgmental operation involves a kind of projection on the part of the clinician who implicitly says, as it were, "he is like me" or "he is different from me." This may be the operation which underlies most judgments which we call intuitive and may be accurate in many instances; we know, however, that it can be grossly false, especially when emerging from persons who are suffering from pathological motivation. As a fourth class, Hathaway sets aside a judgment (pure intuition) distinguished by the fact that the rational and the informational basis for the judgment is unknown. This study suggests that the power of a limited amount of information may be much greater than commonly supposed. Taft (32) provided a general review of the ability to judge people and included a useful bibliography of 81 titles.

Bieri (33) found that persons with meager cognitive resources for describing other persons tend inaccurately to ascribe their own attributes to others. Persons whose cognitive approach to the appreciation of other people is complex are inclined to be accurate in specifying differences between themselves and others. In a collaborative study with Blacharsky & Reid, Bieri (34)

uses Sullivan's notion of parataxic distortion (35) as a factor in the accuracy of judging or predicting the behavior of others and Cameron's notion (36) that maladjusted persons protect themselves by an assimilative projection wherein they perceive others as similar to themselves. The results of this study are inconclusive. Related to Bieri's study is one by Halpern (37) wherein it is shown that subjects may be able to judge others most accurately with respect to attributes which are satisfying in themselves.

In a new area of inquiry, such as the judgment of others, inconsistencies are inevitable. Fortunately, articles are appearing which explicitly indicate some of the artifacts that may creep into the results. The adjustment of the predictor, as well as the adjustment of the predictee, are obvious complicating factors. Wittich (38) reports a correlation of .73 between the adjustment of the predictee and the scores his associates predict for him. Cline (39) studied the characteristics of predictors and finds that the accuracy of judges varies not only with the judge but with the person being judged. The fact that professional consensus does not necessarily confer validity is noted by Morris (40) and suggests that areas where we judge most confidently should be included in our scrutiny. Although the literature includes numerous studies where some attempt is made to predict the manner in which an individual may answer questionnaires, they are not predictive studies in the usual practical sense. Nevertheless, this literature is likely to embolden psychological investigators in their efforts to see what is involved in the more practical types of predictions.

ATTEMPTS AT PRACTICAL PREDICTION

Studies describing practical individual assessment predictions are relatively rare, but they do exist and probably will become increasingly common. This is an important development because the design and reporting of predictive studies will not only refine our instruments but also sharpen our concepts. It is quite possible that we may eventually learn as much about the nature and conditions of behavior change through this approach as we have learned in the past through observing the conditions under which rats may be induced to select one or another mode of response in approaching their food. Cowden, Deabler & Feamster (41) were interested in two predictive questions: whether various projective tests would show a significant change as the patient improved, and whether the tests might be used to predict the adjustment of the patient outside the hospital. It is a commentary on our preoccupation with psychopathology that the patients who would not get better were predicted with greatest success.

Duration of the illness, a well-known factor in prediction, has been employed by Sonder (42) who shows that the outlook for chronic patients with good performance may be relatively unsatisfactory whereas the outlook for acute patients with good performance may be satisfactory.

Attempts to use projective tests predictively have not met with easy successes and the reports are instructive in that they suggest possible modi-

fications in our predictive emphasis. Vernier and collaborators (43) used a battery of projective tests in an attempt to identify the patients who would leave a tuberculosis hospital against advice. Their results draw the reader's attention to the importance of having a specific well-conceived analysis of the behavior to be predicted and of selecting projective tests in a manner certain to include the most relevant motivational and coping attributes of the individuals. Filmer-Bennett (44) tried to show that psychologists could use the Rorschach in predicting which patients will improve from treatment. It is possible that the general lack of success in this study could be attributable in part to variation in the type of treatment the patients received. Becker & McFarland (45) describe a scale which on a postdiction basis distinguishes between patients who may improve from lobotomy and those who will not; their findings confirm some of the earlier work of Mettler and his collaborators (46). One of the most important studies was contributed by Hilton *et al.* (47). This quantitative, systematic study shows a healthy desire among professional psychologists to assess their own work and should be read by all serious students of the theory and technique of assessment.

DESCRIPTIONS OF SPECIAL GROUPS

Patients with neurological defects.—Many of the testing devices that have been proposed as aids in identifying neurological disorders have not stood the test of time. Nevertheless, the year 1955 has produced a new show of strength and interest in this area of inquiry.

Pruyser & Folsom (48) examined Rorschach's suggestion that there should be an overemphasis on color in epileptics. They found that the epileptics' protocols were characterized by constriction which was more marked with respect to M than with respect to C. If one were to consider only the *erlebynistypus*, one would find that the epileptics were relatively extratensive; this is hardly an overemphasis on color, however.

In an exploration of differences between posttraumatic organic, psychotic, and neurotic cases, Hertz & Loehrke (49) found that the organic group was more distinctive with respect to configurational aspects of the Rorschach than with respect to discrete scores. Fisher & Gonda (50) compare Rorschach results with neurologic criteria, and Orme (51) used the Rorschach along with several other clinical instruments in making a comparison between groups of older patients. The study employed a succinct summary for the results of a rather elaborate testing procedure. The authors claim that their materials can be used to distinguish between senile dementias and depressed patients of advanced age. Coons (52) compared a group of schizophrenics with a group of organics. Coons, like others, noted that schizophrenics as a group are likely to be distinctive because of the uncertain quality of their motivation. The organic patient's motivation to do well may be so intense as to detract from his performance. Morrow & Mark (53) used the Wechsler-Bellevue to compare brain-damaged patients with psychiatric patients. There were no reliable differences with respect to such subtests as information, comprehension,

and vocabulary. There was reliable loss in such tests as digit symbol, block design, digit span, arithmetic, and similarities. A similar study is reported by Beck & Lam (54) who examined the possibility of using the WISC in detecting patients with organic disorders. The authors conclude that there may be no characteristic broad pattern for organic patients and that probably experience is the best aid to the examiner who desires to use psychometric results in order to diagnose an organic condition. Using 300 successive admissions, Cohen (55) examined the value of a diagnostic pattern analysis of Wechsler's subtest scores. Many of the psychologists in his sample could not use the subtest patterns in order to make diagnostically accurate statements. Extreme deviations on the subtests may be useful aids in diagnosis, but obviously may not be of equal value to all psychologists. Gunzburg (56) shows that the correlation between the Wechsler and the Goodenough, so conspicuous for mental defectives who do not suffer from pathology, is reduced for subjects with pathology. This shift in correlation is consonant with the belief that pathology, particularly organically based pathology, has an effect on the pattern of intraindividual differences.

The psychologist's diagnostic approach has long employed the supposition that the organic patient is incapable of forming abstractions. Hopkins & Post (57) examine the value of this proposition by employing a battery of several well-known tests of abstraction. Many of their young patients without organic disorders were unable to assume the abstract attitude and were characterized by a concrete attitude. This would suggest, therefore, that differences indicated by tests of abstraction are diagnostically useful in the limited sense that a good abstract attitude makes the presence of an organic disorder unlikely.

The Bender-Gestalt procedure is a hardy favorite. One of the encouraging studies is provided by Bowland & Deabler (58) who show that the performance of organics is much worse than the performance of the other groups. A more comprehensive study of the Bender-Gestalt is provided by Mehlman & Vatovec (59). They are concerned with the possibility of using the test to distinguish between institutional psychotics without brain damage and persons with brain damage. The authors were very cautious about the practical value of this instrument for this type of diagnostic purpose. The best judge was correct in 18 of the 25 pairs. This is a large margin of error, particularly when one remembers the gross nature of the diagnostic task. There were other attempts to identify organic patients. For example, Strauss & Lehtinen (60) had claimed that they could distinguish endogenous from exogenous mental defectives. Using the devices of Strauss & Lehtinen, Rafi (61) found no evidence that this procedure could provide a basis for distinguishing between the mentally defective and the brain damaged.

Since a large number of clinicians may not be able to use familiar devices in a reliable way for a large number of their cases, promising results for novel devices should be explored. Using a new procedure, Reitan (62) finds some extraordinary distinctions between individuals with brain damage and those

without. Price & Deabler (63) used the spiral after-effect as a device for detecting organic brain damage. The spiral after-effect was reported by all of the normal and the nonorganic patients, but 60 per cent of the organics failed to report this effect.

Behavior disorders.—The use of tests to distinguish between persons with severe behavioral disorders has provided a bewildering literature. Studies which are primarily descriptive suffer most from an absence of suitable behavioral criteria. Although several investigators, particularly Wittenborn (64) and Lorr (65), have sought to prepare criteria for the kind of behavior which comprises a psychiatric diagnosis, psychologists usually improvise a novel set of criteria for every study they make.

Nevertheless, criteria emerging from Lorr's multidimensional scale for psychiatric patients were used by Stotsky & Lawrence (66). They found that performance on the Rorschach, particularly with respect to form, location, populars, and ability to shift determinants, was related to conceptual disorganization as judged independently by rating scales. The ability of the patient to shift his determinants in the Rorschach inquiry is coming to be a part of the broad picture of diagnostic procedures (67). Gibbey *et al.* (68) compared delusional and hallucinatory patients with respect to determinant shift and indicated that the delusional patients were more likely to modify their determinants on the inquiry than the patients who were hallucinative. The possible value of such pragmatic supplements to the diagnostic procedure is illustrated in a report by Dana (69) who found that TAT protocols could be scored with satisfactory reliability and that such scores could be used to distinguish between normal, neurotic, and psychotic patient groups.

An important group of patients where no elaborate identifying criterion is necessary are those who have made suicidal attempts. Sakheim (70) used the Rorschach as the basis for comparing suicidal with nonsuicidal patients. The results confirm the work of Hertz (71) by showing that some of her suicidal configurations distinguish between the two groups.

A report by Davidson, Parnell & Spencer (72) concerned with mental upsets among students at Oxford University was somewhat disappointing in that no important, dependable differences were reported.

Three general books of particular value to the clinician are Thorne (73), Brower & Abt (74), and the *Evaluation in Mental Health* publication (75), a United States Department of Health, Education, and Welfare report which includes a useful annotated bibliography.

CONCEPTUAL VALIDATION

The Rorschach procedure continues to be provocative of research. Many of the earlier studies were of an empirical type wherein the investigator sought to reveal a relationship between Rorschach responses and diagnostic grouping. Currently research interests center about the validation of the conceptual basis for the Rorschach and other projective instruments.

One of the most provocative areas for inquiry concerns the possible signifi-

cance of the human movement response. At a recent professional meeting (76) it was suggested that the meaning of a human movement response may be peculiar to the nature of the stimulus material. This is consonant with a report by King (77) who found no reliable correlation between the Rorschach movement responses and the Levy movement responses.

Bieri & Blacker (78) assumed that the movement responses would involve some sort of an internalized response process which may not be required for the color responses. This is reminiscent of the thinking of Singer (79) and his associates who have been concerned with the possibility that the human movement response may indicate a capacity to inhibit or to delay responses. Bieri & Blacker found that introversive persons required more time than other groups, and those persons who required a long time when giving an M response also required a long amount of time when giving a CF response. It would appear, therefore, that introversive persons require more time regardless of the nature of the response they offer. This could suggest that the meaning of a given response might be different, depending upon whether or not the subjects belonged to a group of extreme introverts or extreme extroverts. This is consistent with Rorschach's suggestion that it is not the response per se that must be judged, but the response of some particular person in the context of his total pattern of responses.

A study by Barron (80) indicates that the tendency to give human movement responses to the Rorschach may be more of an expression of an intellectual disposition than a measure of intellectual ability per se. This is a fortunate distinction because most studies have not been able to show that movement, as Rorschach's indicator of intellectual life, was correlated with intellectual ability. Barron's study shows that persons who readily give movement responses may be described as intelligent and inventive with wide interests and perhaps a mild, somewhat anxious disposition. In a study of college women of high achievement (81) Cox found that those who made their achievements with little personal cost tended to provide Rorschachs which were relatively high in human movement response. Although these results are somewhat consistent with some of the implications of Rorschach's claim for the intellectualizing, reflective significance of movement responses, the nature of the implied human trait is still obscure.

The currently emphasized cognitive, intellectualizing or perhaps need-projecting significance of the human movement response is quite different from some of the suppositions concerning human movement as having a motoric, kinesthetic significance. Graham's (82) study bears a tangential relevance to this latter concept; as an incidental part of a larger study, no relationship could be found between the autokinetic phenomenon and a tendency to give human movement responses to the Rorschach.

It is not necessary for the human movement response to represent some sort of a generalized personality trait in order for it to be of great use to the diagnostician and of interest to the investigator. Hammer & Jacks (83) are concerned with the possibility that a human movement response may be an

important expression of unconscious characteristics. Dividing human movement responses into the Flexor, the Blocked, and the Extensor attitudes, they assumed that the Extensor attitudes are expressive of self-assertion, the Blocked movements indicate indecisiveness, and the Flexor movements indicate compliance or passivity. The prevalence of these types of human movement responses was examined among three groups: rapists who had assaulted adult females, pedophiles who had assaulted a female child, and homosexual pedophiles who had assaulted a male child. The Flexor type movements were much less characteristic of the rapists; these subjects showed more of the Extensor type movements than either of the pedophiles groups. Such relationships as these are a kind of conceptual validation for the notion that the content of human movement responses may be usefully symbolic of underlying attitudes. A study by Mirin (84) tends to add credence to this point of view. He found that patients who were assertive in a social stress situation also tended to show assertive responses in the Rorschach situation. Mirin's study, as well as the study of Hammer & Jacks, clearly suggests that regardless of whether human movement has an implication for a general trait of personality, the personal significance of the human movement response varies according to its content.

The possibility that certain human movement responses have a particular personal significance is examined by Hirshstein & Rabin (85). Using the presence or absence of parental figures in the background of the individual as the independent variable, responses were observed to cards IV and VII, commonly accepted as symbolic of parental figures. Presumably if the individual had no parental figures in his background, he would be less sensitive to parental symbols than a person who has had the usual parental association in his childhood. It was found that the group who had a family background required approximately twice as much time for responses to cards IV and VII as those who had had no stable parental figures. This would suggest that the increased time required for human movement responses may be a result of the assimilation of the form of the blot with the personally significant figures in the individual's background or the images of his own requirements. As a part of the general interest in the meaning of the content of human movement responses, Tolman & Meyer (86) correlated the sex of human figures perceived on the Rorschach and the therapist's estimate of whether the individual identified with the male or female parent. The subjects were all males. No relationship was found. The perceptions of human content generated by the Rorschach cards may be in a sense wish fulfilling, but the nature of the response itself may not be an indication of the nature of the wish that is being fulfilled.

At one time the significance of the color response may have held the center of the stage. It had been assumed that any interference or difficulty in using color was profoundly significant for the emotional adjustment of the individual.

Some minor controversy has grown up between Lazarus and Siipola in

their explorations of the manner in which color affects responses to the Rorschach. Siipola's hypothesis of color-form incongruity as a factor in the significance of color responses (87) was explored by Berg & Polyot (88); their data did not permit them to sustain Siipola's hypothesis. In a recent article, Lazarus & Oldfield (89) confirmed some of their earlier work and concluded that whether a card was colored has little to do with the response time, the affective value, or the content of their responses. This issue received further attention from Hamlin, Stone & Moskowitz (90) who used a card sorting procedure. They reported that colors were sorted with greater speed than forms and that incongruities of either form or color when introduced into a series will temporarily reduce the efficiency of the sorter. In a study with patients these authors found that the incongruous colors decreased efficiency somewhat. This would be consonant with Siipola's claim.

In support of the validity of some common beliefs concerning color responses, Finney (91) reports that assaultive patients showed reliably more C and CF responses than the nonassaultive patients. It is possible that color *per se* may have a significance for hospitalized patients which it does not have for persons who are not suffering from any marked emotional disorder.

The literature includes several additional studies which in one manner or another attempted to confirm certain commonly held beliefs concerning the significance of Rorschach responses. For example, Zelen (92) showed a relationship between level of aspiration in a task situation and number of whole responses. Vernallis (93) showed that teeth grinding with its rather obvious hostile implications is correlated to an important degree with hostility scores based on Rorschach content, and Shatin (94) reported a study relating the Rorschach to the TAT.

Meer (95) described the differences in the Rorschach cards with respect to the difficulty in responding to them. Lofchie (96) showed that there was an interesting relationship between the perceptual maturity score for Rorschach responses and a susceptibility to distraction on the Dunlap Steadiness apparatus. The ability to integrate disparate sources of stimulation in order to provide a well-balanced pattern of response may be related to a quality of behavior once described as attention (97).

Several investigators have sought to examine the relationship between overt behavior and some of the possible implications of TAT responses. For example, Gluck (98) correlated the aggression expressed by patients in a frustrating and abusive task situation with hostility scores based on TAT responses. No reliable relationships were found. Meyer & Tolman (99) sought a correspondence between attitudes concerning parents expressed in psychotherapeutic interviews and in the TAT protocols. There was no indication of a correspondence.

It is inconceivable, however, that attitudes ascribed to figures invented in response to the TAT are irrelevant and not a part of some aspect of the personality of the individual. Kagan & Mussen (100) suggest that if the motive is culturally sanctioned, a relationship between the TAT elicited phantasy

and overt expression of the motive is likely; if the motive involved is not culturally sanctioned, such a relationship is unlikely. Mussen & Naylor (101) found a relationship between TAT phantasy aggression and overt aggression, but this was for lower class boys where aggressive behavior may be sanctioned. Pittluck (102) also found some conditional relationships involving aggressive behavior, and Lesser (103) in an unpublished dissertation has shown that parental approval of aggressive behavior is an important factor in its appearance. In the Kagen & Mussen investigation dependency themes from the TAT were correlated with a yielding, dependent type of behavior in a group situation. Since yielding, dependent behavior is culturally sanctioned, it was supposed that a direct relationship could emerge between the yielding behavior and dependency themes on the TAT. Some very high correlations were found for a sample of 27 subjects.

Further inquiry into the conditions under which motives inferred from TAT stories could find expression in overt behavior is provided by Davids *et al.* (104). In general the writers suggest that if there are outward channels for the expression of aggressive needs, there is likely to be a correlation between aggression expressed on the TAT and overt aggressive behavior. The implications of this study seem to confirm the implications of the Kagen & Mussen inquiry and clearly indicate that caution should be observed in ascribing overt behavioral significance to aggressive needs expressed on the TAT.

Readers interested in the broad validity and the extending implications of some of the thinking involved in the clinical use of the TAT will be rewarded by a little book by Leonhard (105), *Consumer Research with Projective Techniques*.

PERSONALITY CONCEPTS USEFUL IN ASSESSMENT

Numerous studies of personality and character have a great interest to students of assessment. Some have a broad sociologic orientation, others have a specific diagnostic frame of reference. One by Guertin (106) has to do with the factor analysis of intercorrelations between 29 hospital patients with various types of schizophrenic diagnoses. The basis for the correlations between these patients was provided by an activity rating scale. This transposed matrix of correlations was factored by the usual methods and five factors were extracted. The classes of patients indicated were designated as schizophrenic sequence, withdrawal, disorganization, resistance, and psychotic reorganization. These factors are seen as similar to the types proposed by Jenkins (107). Cameron (108) proposes a functional type of diagnostic categorization for children. He suggests that at least three concepts always be employed: the developmental, the reactive, and the unique features of the child including the idiosyncratic symptoms which characterize him.

Some of the possible descriptive economies of psychoanalytic thinking have been reviewed. Stagner and collaborators (109, 110) used a personal preference scale prepared by Krout (111) in order to identify students who

were most distinct with respect to the various levels of development. After students representative of each level of psychosexual development had been identified, each student was correlated with every other one on the basis of the items of the test. The clustering did not conform with the anticipated types.

Numerous studies of the authoritarian personality have appeared, and some of these are of interest to the student of assessment. Jones (112) found that authoritarian Navy recruits tended to react to leaders positively and tended to prefer an autocratic leadership. It was not found, however, that they were more rigid than the nonauthoritarians. Stotsky (113) reports that no correlation could be found between the Fascism scale of authoritarianism and anxiety as measured by the Taylor Manifest Anxiety Scale (114). Lyle & Levitt (115) examined the possible relationship between the punitiveness of the parents and the ethnocentric, authoritarian attitudes of children. The clinical value of the concept of authoritarian personality is not yet clear.

Another descriptive construct is the alienation syndrome. Davids (116) attempts to relate this syndrome with the cognitive process. He considers the alienation syndrome to be a kind of motivational complex which involves such personality dispositions as egocentricity, distrust, pessimism, anxiety, and resentment. The study employed a perceptual situation comprising an auditory projection technique. It was found that those individuals who were judged to be high on the alienation syndrome were also the ones who were selectively responsive to the negative materials in the auditory test. This study is of interest both because it suggests that it is possible to assess a negative disposition through interview and autobiographical material and because it suggests the development of an auditory procedure which might be used to assess negative dispositions among persons from whom interview and autobiographical material may not be available. Anger and anxiety have long been accorded a place of honor in our studies of the individual, but neither has been subjected to a searching scrutiny; descriptive analyses cast new light on the implications of these constructs (117, 118).

Keehn (119) uses one of the Hartshorne & May procedures (120) in order to identify cheaters and finds that three times as many cheaters were inclined to be high on both neuroticism and extroversion as the noncheaters. This tends to confirm one general stereotype of a cheater as a person who is superficially brazen, but basically insecure. A useful discussion of delinquents from a subcultural point of view is provided by Cohen (121). As has been true for years, the literature abounds in general discussion of interesting personalities, e.g., the compulsive eater (122).

Descriptions of persons who suffer from certain classes of psychological disorders may have considerable incidental value for assessment. Mueller & Lefkovits (123) indicate that persons hospitalized with rheumatoid arthritis are different from hospitalized neurotics in the particular respects that they are likely to be intrapunitive or perhaps impunitive. The arthritics seemed to be characteristically disturbed in their performance on cards IV and VII

of the Rorschach, suggesting that perhaps a part of their problem is the continuation of conflicts concerning parental figures. Bernstein & Chase (124) use Blacky pictures to compare peptic ulcer patients with patients having psychosomatic disorders not involving ulcers. The authors did not find the distinctions previously reported by Blum & Kaufman (125). This is consistent with the results of an inquiry by Streifeld (126) who did not find that elements of oral eroticism could provide a basis for distinguishing ulcer patients. Winter (127) indicates that peptic ulcer patients may not be subject to any oversimplified description. He attempted to find evidence for two types: those who are reacting to their dependence and those who are primarily dependent. Waxenberg (128) compared asthmatic women, women with ulcerative colitis, and women with malignant tumors by using a battery of projective tests. None of these tests provided evidence of reliable difference between the three groups.

King & Henry (129) provided an interesting study of cardiovascular reactions. An earlier report by Funkenstein & King (130) showed that individuals who expressed their anger outwardly had a nor-epinephrine type cardiovascular reaction, whereas persons who expressed their anger inwardly had an epinephrine-like reaction. Funkenstein & King had also found that depressed persons secreted an epinephrine-like substance in reaction to stress. This can be readily tied to the psychoanalytic notion that a depression is an expression of anger toward the self. In the present inquiry, King & Henry examine the relationship between strictness of child-rearing practices and the characteristic form of the cardiovascular reaction to stress. When the father was mild and the discipline not strict, the cardiovascular reaction to stress was of the epinephrine type, whereas a nor-epinephrine type reaction characterized the cardiovascular reaction to stress among persons whose father had been a dominant and strict disciplinarian.

Semmes *et al.* (131) described the spatial orientation of patients with cerebral injury and reported that injury or defect of the parietal area had the most reliable and most definite detracting effect on spatial orientation. Wood (132) compared left and right hemiplegics with respect to perception of figure-ground relationships.

In some respects two of the most interesting studies were provided by Reznikoff (133) and by Wheeler & Caldwell (134). These studies of women suffering from various types of malignancies show that there may be a relationship between the type of malignant reactions in women and their psychological dispositions. Reznikoff's group of women with definite cancer of the breast tended to be characterized by some disturbance in their feminine identification, a childhood history which involved excessive responsibilities, and a history of qualified success in marriage. Wheeler & Caldwell compare their study with earlier reports concerning patients with tumors of the breast and cervix by Tarlau & Smalheiser (135) and with a study by Bacon, Renneker & Cutler (136) concerning cancer of the breast; they are unable to confirm all the results described by these earlier investigators.

Nevertheless, the Wheeler & Caldwell report bears an interesting relationship to the Reznikoff report in that they find that women with cancer of the breast have been less active sexually than either women with cancer of the cervix or a control group of women not known to have any kind of malignancy. It is probable that much may be learned from the study of self-concepts and the development of physical symptoms (137). Possibly self-concept and the personal meaning of life may be shown to be related to general physical expression (138).

Proceeding now from discussion of individuals who are distinctive with respect to their symptoms, either behavioral or somatic, let us turn to studies of individuals who are distinctive with respect to the role they play in society. In a comparison of two groups of students, one judged by teachers to be creative and the other not creative, Drevdahl (139) found that the creative ones tended to be verbally fluent, flexible in their outlook, somewhat radical in their values, and self-sufficient. In addition he indicated that they might be more sensitive emotionally than the noncreative students. This would suggest that the creative are more conspicuous as individuals than the noncreative. Further descriptions of creative personalities are provided by Cattell & Drevdahl (140) who describe researchers. It seems probable that we have much to learn about the exact nature of the distinctions that must be made between an individuality which is psychopathic and negative in its social significance and an individuality which is desirable.

Miner & Culver (141) find that executives need to rely on other people, fear illness, and feel helpless when confronted by a problem where they have no help to draw on. This might suggest that a conspicuous degree of self-reliance may be a liability to one who is being groomed for an executive position.

Harrison, Tomblen, & Jackson (142) compared 100 mechanical engineers with 100 nonengineers. Their data indicate that engineers are characterized by drive, concern for detail, frankness, and feelings of self-sufficiency. They are casual in their interpersonal relationships, take no analytical interest in other people, and are straightforward and unimaginative outside of their own particular field of activity. They like well-structured problems and show interests which are characteristic of men. The results of the studies are compared with those of other investigators, such as Moore & Levy (143) and Steiner (144). The Miner & Culver report, with its emphasis on the tendency of executives to sense their dependence on others and to be inclined to gauge others, provides an interesting contrast with the description of engineers by Harrison *et al.* with its emphasis on self-sufficiency and matter-of-fact relationships with others.

There is a growing appreciation of the significance of school personnel and a useful review of studies of teacher efficiency has been prepared (145). The most comprehensive assessment study of the year made primary use of personnel in an educational situation (146).

EXPLORATION WITH SPECIFIC INSTRUMENTS

There are numerous publications which are rather difficult to classify either from the standpoint of their approach or from the manner in which they relate to systematic, theoretical, or procedural problems. Nevertheless, they are of interest to the student of assessment because they describe applications or modifications of familiar instruments.

Eschenbach & Borgatta (147) studied the relationship between Rorschach scoring categories and independent evidences of overt behavior. In view of such results one is inclined to wonder about the intrinsic sources for the valid use of the Rorschach. It is possible that the value of the instrument may not lie in simple empirical relationships but may emerge from the kind of thinking which clinicians do as they employ the cues provided by the Rorschach. Nevertheless, it should not be assumed that broad diagnostic distinctions cannot emerge from empirical use of Rorschach responses. O'Reilly (148) selected and scored Rorschach responses, and the results suggest that the procedure may be an effective device for identifying psychotics. Taulbee (149), in another one of the studies which has to do with the possible relationship between the Rorschach and intellectual functioning, reports that the accuracy with which judges may assess intelligence from the Rorschach varies greatly from judge to judge. Eron (150) found the Rorschach to have no practical value in selecting medical students. The literature abounds in useful exploration of the Rorschach (151, 152); particularly to be noted is a study by Levy and collaborators (153).

Studies which involved clinical procedures for the assessment of mental ability were characterized by a considerable diversity. Barry and collaborators (154) examined the comparability of forms I and II of the Wechsler-Bellevue scales when both forms were administered to the same sample of patients. They not only report the expected practice effect, but also indicate that the correlations of the subtests from one form to another were so small that one must be most cautious in substituting a subtest from one form of the test to the other. [Incidentally, an interesting study of the effect of training in ability is provided by Blade & Watson (155)]. Investigators have also been interested in comparing the Wechsler-Bellevue with the WISC. Cole & Weleba (156), using a sample of college students, show rather small correlations between these two tests of intelligence. Goolishian & Ramsey (157) compare the Wechsler-Bellevue with the WISC in a procedure which involves the use of two comparable groups. Their data suggest that the Wechsler-Bellevue may yield higher intelligence quotients than the WISC.

A well-conceived and commendably thorough standardization of the Wechsler Adult Intelligence Scale is provided for older persons by Doppelt & Wallace (158). Numerous instructive tables provide complete distributions and intercorrelations for different age levels.

Some of the limitations in using short forms of the WISC were examined by Yalowitz & Armstrong (159) whose subjects were children with mental

and emotional handicaps. In general, the correlations they report and the correlations they cite from other investigators are rather low. Stacey & Carleton (160) examined relationships between the Ravens matrices, the WISC, and the Stanford-Binet. Their samples were children of limited ability and their results suggest that the matrices bear a higher correlation with the Binet than they do with the WISC. Stacey & Gill (161) describe a similar investigation based on a sample of adult subjects of subnormal intelligence, and in general it was found that the matrices test bears a higher correlation with the Binet than it does with the Wechsler.

The significance of background factors for intelligence has long been of interest and has resulted in broadened concepts (162). It is assumed by many persons that a typical test of mental ability is more favorable to a middle class child than to a child of lower class origins. Rosenblum and collaborators (163) report the performance of lower class retarded children on the Davis-Eells Test. Performances were compared with respect to the Davis-Eells Test, the Binet, and the WISC. No superiority on the Davis-Eells Test was reported despite the lower class origins of the children.

The Lowenfeld Mosaic Test (164) has also been the object of some inquiry, and Wideman (165) provides a comparison of schizophrenics, psychoneurotics, and organics. Stewart & Leland (166) compare the Lowenfeld Mosaic Test with other tests of intelligence. Apparently intelligence may have little to do with the type of mosaic that the children produce.

A study by Dana (167) considers the relative value of different subgroups of TAT cards. His results indicate that the meaningfulness and the discriminative value of material coming from the TAT may be more of an attribute of the person who is telling the story than it is a function of the exact nature of the cards which provide the stimulus for the stories. Lindzey & Heine-mann (168) provide an instructive inquiry into the possible implications of group and individual administration of the TAT. They find great consistency between group and individually administered tests. Zimmer (169) reports low interjudge reliability for judgments based on the sentence completion test and the figure drawing test and very low validity coefficients. Churchill & Crandall (170) suggest that the incomplete sentences procedure may be a reliable instrument and that the norms are meaningful and appropriate. Apparently the indications of validity leave something to be desired, however. Evaluative comments are also made for the Bender-Gestalt (171) and the Blacky Pictures (172). The reports of Scott (173) and of Pearl & Jacobs (174) concerning the Szondi are not fully encouraging. It would seem that the literature encourages us in the use of projective instruments when we seek to find consistency based on theoretical consideration. If, however, our search for consistency is based upon a purely pragmatic relationship, most of our inquiries are discouraging. The Minnesota Multiphasic Inventory has proved itself to be a hardy perennial and seems to have generated a wide variety of empirical researches (175, 176).

There are two articles concerned with the word association procedures. One by Weider *et al.* (177) involves further developments of the Cornell word

form; the second paper by Riklin (178) describes a word association demonstration for a 28-year-old man.

A study by Secord & Muthard (179) described a procedure wherein groups of judges tended to rate pictures of young women in a highly similar manner with respect to several different personality attributes. The validity of the judgments is not discussed, but it is apparent that we tend to ascribe attributes to certain kinds of faces in a rather consistent way. A study by Tomkins & Miner (180) involved the use of a Gallup Poll crew in order to get normative data for the Tomkins Horn Picture Arrangement Test.

A diagnostic study of children by Bosquet & Stanley (181) is of interest because it indicates that there are no important differences in Rorschach scores among children ranging in age from 7 to 13. The gross validity of the children's form of the picture frustration study was explored by Levitt & Lyle (182).

O'Connor, Lorr & Stafford (118) provide a long-overdue analysis of the items comprising the Manifest Anxiety Scale. The use of this anxiety questionnaire comprises something of a phenomenon in recent psychological literature and whether all of the investigators who have employed this device either as a predictor or as a criterion believe it had some singular behavioral significance is a matter for conjecture. In the present report, however, the anxiety items were intercorrelated and submitted to a conventional factor analysis. Instead of one factor of anxiety, five identifiable factors emerged. Thus it would appear that scores on the anxiety scale represent not one, but five different kinds of human reactions and the question may legitimately be raised in the case of any one score as to which of the factors it may most express.

Perhaps the biggest news of testing instruments is the revision of the Wechsler (183). Many desirable changes and extensions are incorporated with no apparent detractor from the merits of the original instrument. The Strong Vocational Interest Blank, long used in assessment, is recognized by two fine volumes (184, 185). Small has prepared a useful location and scoring manual for the Rorschach (186), and several promising new procedures have appeared (187, 188, 189). Phillipson (190) has provided a book describing the use and clinical possibilities of the Object Relations Test.

APPROACHES TO ASSESSMENT

There is an interesting body of material which has to do with general methodologies for inquiry and assessment. Some of these methodologies are perhaps more of an expression of a point of view than they are an expression of a refined rationale. Leary & Harvey (191) describe a methodology for measuring personality changes in consequence of psychotherapy. Possibilities of getting a quick and pervasive summary of the consistencies and inconsistencies within a person's attitudes are described by Toman (192), who offers a procedure which in some respects is reminiscent of some of the efforts of McQuitty (193).

A study is offered by Friedman (194) to indicate a distinction between

the true self, the ideal self, and the projected concepts concerning the self. In keeping with Rogers' hypothesis, there was a higher correlation between these three sets of concepts for normals than there was for neurotics. The concept of an ideal self is explored further by McKenna *et al.* (195) who compares *Q*-sorts for the ideal self with *Q*-sorts for a friend. Subjects' personality pictures for a friend closely resembled the personality picture for the ideal self. A useful bit of information comes from a study by Jourard & Remy (196), who show that there is a conspicuous correspondence between the manner in which a person perceives himself and the manner in which he believes his parents perceived him.

Role playing behavior continues to command the interest of psychologists and a study by Borgatta (197) indicates that there is an important correspondence between actual behavior as observed through a one-way mirror and role playing behavior. Helfand (198) studied the role taking behavior of schizophrenics and was able to show that schizophrenics were less successful than other persons in sorting items in order to simulate the *Q*-sort that would be provided by a person whose autobiography they had read. Some of the schizophrenic patients were extraordinarily sensitive, and the author suggests that some schizophrenics may lack a concept of the generalized other person, and in this way they may have a hyperacuity for differences between people. Using stick figures as stimuli, Sarbin & Hardyck (199) compared normals with schizophrenics on the basis of consistency in the perception of roles. It was found that normals tended to be pretty consistent in the roles they ascribed to the stick figures, whereas the schizophrenics were quite diverse in the roles they perceived. Such trends bear an interesting congruence to the commonly observed tendency of patients or other maladjusted individuals to ascribe atypical, if not deviant, motives and behavior to others.

The value of the interview as an instrument for assessment receives some specific attention. Yonge (200) provides a review of the use of the interview in assessing behavior and suggests that the interview can be a valuable aid. Wispe & Lloyd (201) describe a use of the interview in their study of the role that the district manager should play in relation to his insurance agents. Giedt (202) made a systematic, experimental attempt to learn more about the interview as a basis for judgment. Luck's (203) *Personnel Audit and Appraisal* will be of interest to those who do assessments for industry.

Psychologists are also concerned with the appropriateness of their communications. Articles include a study by Gottschalk & Hambidge (204) and a paper by Thorne (205) presenting an outline for psychological report writing. In his outline as in his *Principles of Psychological Examining* (73), Thorne has emphasized the importance of assessment at different levels of personality integration. Cuadra & Albaugh (206) analyzed some of the sources of ambiguity in psychological reports and examined the correspondence between the author's intention in preparing a psychological report and judges' interpretations of the meaning of the report. The authors describe a

correspondence of only 53 per cent. Tallent (207) is also concerned with improvements in psychological report writing and suggests a scheme of research for improvements in this area.

A study of Datel & Gengerelli (208) is most apropos here. Their procedure amounts to a kind of Rorschach interpretation matching contest among a group of clinical psychologists. The task of the psychologist was to match the interpretations with the appropriate original protocol from which the interpretation was derived. The authors conclude that a lot of Rorschach reports may have little communication value. Foulds (209) established a scheme whereby the agreement and disagreement between psychiatrists and between psychiatrists and psychologists' diagnostic reports can be graded. His report is optimistic about the reliability of psychiatrists' evaluations and the validity of psychologists' diagnosis.

CONCLUSION

The preceding paragraphs comprise a digest of the periodical literature which appeared between January 1, 1955 and April 1, 1956. The topical organization seeks to indicate some of the areas of activity and does not, therefore, classify on the basis of theories or methods. The reviewer sought to emphasize the conceptual refinements and procedural innovations which would be of interest to those who assess the individual. No attempt was made to review devices which have been developed in the psychometric tradition for use in group situations. Of necessity, reviews, textbooks, and test revisions received little attention, particularly if they did not appear to be distinctive to the period.

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COUNSELING^{1,2}

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A major psychological contribution of 1955 is found in Kelly's proposition (44), which he elaborates into a comprehensive theory of personality, to the effect that man construes events. A corollary to this proposition is that natural events are subject to alternative constructions. The literature on counseling is no exception, and the concept of counseling itself depends on the construer! While abundant precedent can be found for construing counseling as something different from psychotherapy or for distinguishing between counseling psychology and clinical psychology, as in Hahn's 1954 presidential address (37) to the Division on Counseling Psychology of the American Psychological Association, alternative and higher-order constructions are suggested by Sanford's concept of creative health (73) or Super's concepts (88) of vocational development and maturity. By means of such concepts mental illness can be comprehended as an interruption or retardation of growth. If mental illness is considered the province of the clinical psychologist and creative health the domain of the counseling psychologist, but both are construed as developmental phenomena, then counseling psychologists and clinical psychologists share the common enterprise of what might be called developmental counseling.

This developmental perspective can be discerned in papers appearing in 1955 (17, 58, 82, 87) written by counseling psychologists vitally concerned with the rehabilitation of the mentally ill. Their interest, it would appear, is not so much in "curing" or "treating" as it is in opening up new opportunities for constructive life experiences and maturation of personal resources. The implications of this kind of interest for psychotherapy as a process whereby the individual experiments with altered relationships to, or new modes of living in, his environment cannot be escaped even though the counseling psychologists exhibiting this interest do not profess to practice psychotherapy. The conceptual trends emerging in 1955, then, by-pass distinctions between counseling and psychotherapy or counseling psychology and clinical psychology, distinct as some of the functions of "counseling psychologists" and "clinical psychologists" may be. The refreshing prospect of begging the question as to how such distinctions should be handled is, therefore, offered. Such being the case, precedence is given in the following review to theoretical and conceptual trends without any presumption that these trends apply in any exclusive way to counseling psychology in contradistinction to clinical psychology or psychotherapy. This is followed by

¹ The survey of the literature pertaining to this review was completed in April, 1956.

² The following abbreviations are used in this chapter: MMPI (Minnesota Multiphasic Personality Inventory); TAT (Thematic Apperception Test).

sections on counseling practice, studies of the counseling process, measurement, and vocational and rehabilitation counseling.

THEORETICAL AND CONCEPTUAL TRENDS

The interest of counseling psychologists, spoken of above, in opening up new opportunities for constructive life experiences and maturation of personal resources corresponds to Murphy's plea (57) for the "social science approach." This approach, Murphy says, implies "a fuller understanding of situations open to the client, so that situations can be found which are releasing to him." Such an approach departs from the early psychoanalytic precedent of presuming to make "changes inside the person guided, almost in the manner in which the surgeon removes something" and is instead analogous to the conception of medicine which looks for "situations in which a person can function so that nothing has to be incorporated within his body, but only an opportunity given him to live." Super (89) offers what might be considered an attempted implementation of Murphy's social science approach in his hypotheses that (a) it is possible to release the individual's ability to cope more adequately with other aspects of living by helping the individual to understand and cope with one important area of adjustment, i.e., the vocational, and (b) that this assistance is best accomplished by building on the individual's assets. Super goes on to say that "some clinicians, but too few, share this (positive) approach," and maintains that "if the clinical and counseling specialties can in due course merge as one broad and well-balanced specialty, it will be because some psychologists have chosen to point up the possibilities of the positive approach by calling it counseling rather than clinical psychology."

Evidence appears in 1955 of two trends in conceptions of the counselor's role. The first points up the counselor's role as active and definitive. Martire (49), for example, argues that "the counselor can provide a more adequate object for identification than a random person in the client's environment." Williamson (97 to 100) in somewhat similar vein holds that counseling provides opportunity for a nonpunitive fostering of internalized authority which he sees as one important element in self-confidence. A second trend in conceptions of the counselor's role stresses variation as in Robinson's suggestion (67) that the counselor plays different roles in response to client and field differences and makes adjustments within these roles "by varying his acceptance attitude, responding or not to the core of the client's remarks, altering the division of responsibility, and modifying the degree of lead in his remarks." Bordin (13) sees the counseling relationship as one that varies along different dimensions such as ambiguity and emotional tone; implicit in this view is the hypothesis that effective counseling requires variations in the counselor's role from client to client as well as from time to time in a relationship with one client. Shaw's description (76) of the counselor as a strategist who "'moves in' on the client's expectancies in accordance with whatever opportunities he has" combines the active and flexible conceptions of the counselor's role.

If theories have their antecedents in general conceptual trends such as

those mentioned above, and if the trends noted antedate 1955, as they undoubtedly do, it should be possible to detect relationships between these trends and theory development. Perhaps such a relationship can be found between what Murphy speaks of as the "social science approach" and the kind of theory that regards the person himself as a theorist. Kelly (44) has advanced such a theory and a view of counseling consistent with what Murphy speaks of as a "fuller understanding of situations open to the client." Kelly regards the individual's "destiny" as a function of his interpretation of events in the same fashion that the predictions emerging from a theory depend upon the postulates of the theory. Alternative interpretations may open up new "destinies" that are not accessible within the framework of a given interpretation, and counseling fosters the trying on of alternative personal frameworks "for size." Shaw (76) follows a similar line of thought in viewing counseling as a process of "up-ending expectancies," a process that requires that "the counselor discern whatever generalizations or personal theories, as George Kelly puts it, underlie these expectancies." Pepinsky (65) speaks of "the utility of the theorist as a model of the person" in understanding clients; this phrase is comparable, of course, to Shaw's reference to discerning generalizations or personal theories. The selection of the theorist as a model of the person is also the very selection that Kelly made in constructing his theory. The influence of theory-making upon the theoretical efforts of counselor-clinicians is very much in evidence in 1955, since it has itself been employed as the model for theory. Conceptions noted above of the counselor's role as active as well as flexible would appear to be consonant with the view of the individual as a theorist, a view that implies an active exploration of alternative personal theories as well as a flexible approach to the means by which this is accomplished.

COUNSELING PRACTICE

A review of counseling practice is more likely to enlarge counselors' understanding of what they are doing if it points up broad but at the same time practical problems rather than enumerating specific techniques without reference to broader issues. Anderson & Bown (1) reflect this view in their contention that the counselor's specific technique of interviewing is of little importance but that the intent of communication is. They present an approach to counselor training through the use of tape recording and set up as one of their aims the avoidance of technique orientation on the part of the counselor-trainee. Bordin's description (13) of the counseling relationship as one that varies along different dimensions is helpful in grasping counseling as a process rather than as a set of techniques, since a consideration of the dimensions requires the counselor to deduce his techniques rather than employ them as rules-of-thumb. The tenuous contact of a schizoid patient, for example, might dictate the use of techniques that minimize ambiguity in the counselor's participation and maximize his contribution to planning in the cognitive-conative sphere as well as his involvement or commitment as a quality of the emotional tone of the relationship.

A large aspect of the practical problems confronting the clinician or

counselor has to do with the making of decisions such as whether a patient should be committed to a hospital, whether vocational training of a particular kind should be recommended for consideration, etc. DuMas (24) offers comfort to the counselor who is reluctant to "play God" by pointing out that a three-value logic (false, doubtful, true), employed when a decision is withheld, and a two-value logic (false, true), employed when a course of action is initiated, may be regarded as three-intervals and two-intervals, respectively, of the probability continuum. One need not, therefore, abandon probability in favor of infallibility in order to make decisions.

Another aspect of the practical problem confronting the counselor concerns the inferential potential, as Levy, Brody & Windman (47) speak of it, of his diagnostic instruments. They had judges rank 12 Rorschach and TAT protocols on the basis of their relative inferential potential and found a significant relationship between the two tests which they interpreted as indicating that the individual S accounts for a major portion of the variance in inferential potential. They suggest that to the extent that their findings may be generalized, they fail to support the assumption that the more test results available on the individual the more one is likely to learn about him.

Cronbach (20) identifies a problem that is fundamental to counseling practice, precision versus breadth of information, by concepts drawn from information theory, i.e., bandwidth and fidelity. A counselor might secure accurate information about questions he considers important and thereby sacrifice range of information for fidelity or precision, or he might "roam" widely in order to get leads to important problems, a procedure which may be fruitful for purposes of "canvassing" but one which is likely to sacrifice a high degree of faithful information about any one area. Risk in seeking precision is minimized when the problem is familiar to the counselor or when there is good reason to believe that certain facts are much more relevant than others. Emphasis on bandwidth, on the other hand, is preferable when the counselor does not have the necessary experience to narrow his focus or when it is impossible to specify only a few relevant areas in advance.

Bauernfeind (5) presents an unorthodox but persuasive case for dispensing with sex norms, a proposal which is likely to sound revolutionary to counseling practitioners. He composes a brief which would command the respect of any trial lawyer in which he argues that (a) expanded systematic knowledge can be achieved by reports of means, standard deviations, and the results of significance tests without publication of detailed sex norms; (b) no norms are needed on test scores functioning as dependent-variable data when such data are presented as raw scores and that separate tables of standard scores by sex can actually confuse communication; (c) norms by sex should be provided within a defined success-group if there are significant differences between the sexes in the group, but that general population norms by sex might only promote confusion by providing a counselor two different points of reference from which to enter tables of success-group norms; and (d) the problem of providing tools for improved educational and clinical diagnosis hinges on the question, "With whom will the person

associate?," and that "in the vast majority of their interactive experiences, most Americans today function in a heterosexual environment."

Of the books on counseling appearing in 1955, Bordin's *Psychological Counseling* (13) represents an original contribution which is likely to leave its mark on counseling practices. Other books of general interest to counselors include *Counseling Psychology* by Hahn & McLean (38), a revision of their earlier *General Clinical Counseling*, and *Six Approaches to Psychotherapy*, edited by McCary & Sheer (53). *A Casebook of Counseling* by Callis, Polmantier & Roeber (14) presents five cases drawn from a university counseling service and *Counseling and Guidance in General Education*, edited by Hardee (39), explores a variety of problems of interest to college and university teachers and administrators as well as to student personnel workers. Aptekar (3) brings a perspective drawn from his field of social casework to counseling in *The Dynamics of Casework and Counseling*. Books of which those identifying themselves with the general area of "guidance" will want to take note are the volumes written by Mathewson (51), Kelley (43), Ohlsen (61), Bennett (7), Smith (78), Roeber, Smith & Erickson (68), and Willey & Andrew (96), and the compilation edited by Andrew & Downing (2).

STUDIES OF THE COUNSELING PROCESS

A decision as to what should be included in a review of studies of the counseling process depends, of course, upon the range of one's view as to how light might be thrown upon this process. Since knowledge advances via the relating of apparent "irrelevancies," it is probably wise to select a view of such range that it is possible to reach into areas of investigation that are at least superficially remote from the topic under consideration. The following review of studies of the counseling process, therefore, includes studies holding implications for the counseling process even though they have not been designed to investigate this process directly or with any intent necessarily of investigating it even indirectly. Studies pertinent to some aspect of counselor participation, methodological studies, and studies of outcome will be considered in order.

Studies pertinent to counselor participation.—An excellent illustration of a study which was not designed to investigate a hypothesis pertaining to counseling but one holding important implications for the counseling process is reported by Lundy (48). He had subjects meet and converse with one another and instructed them to focus attention upon themselves under one condition and upon the other person under another condition. He found increased accuracy of prediction of the other person's responses to Part 2 of the Allport-Vernon Scale of Values under the latter condition. Among the implications of this finding is that the counselor who is presumably ready and eager to focus attention on others is more likely to comprehend and relate successfully to a client. It is interesting to note in this connection that Parloff (63) found that the therapist who was able to establish better social relationships, as determined by the quality of such relationships with 12 mutual acquaintances of subordinate status, also established better therapeutic

relationships. Consistent with Lundy's results also is Parloff's finding that the therapist who perceived a patient as approximating his ideal-patient concept more closely (and, therefore, one upon whom he presumably more readily focussed his attention) created the better relationship with that patient.

Studies by Smock (79, 80) and Beam (6) hold implications for stress as it might be experienced by the counselor, although they were not set up for the purpose of testing effects of stress upon counselor performance. Smock (79) presented a series of cards of decreasing ambiguity to stress and non-stress groups and found that psychological stress tends to result in increased intolerance of ambiguity. In a second study (80) he investigated this hypothesis further by studying the effects of stress on the recognition of incongruity and found that stress is associated with adherence to prerecognition "hypotheses" and retarded recognition of departures from the familiar. The implications of these studies for the counselor's ease as he copes with the unfamiliar and ambiguous aspects of client data are self-evident. Wispe & Lloyd (102) found results consistent with Smock's in a study of preference for structured (unambiguous) interpersonal relationships among sales personnel of a life insurance company. They found a positive relationship between amount of "threat orientation" and desire for structured social interaction. Beam (6) investigated the effects upon serial learning of real-life stress such as that aroused by doctoral preliminary examinations. He found evidence for retardation of learning under such stress as compared to learning under neutral conditions. Although no sweeping generalization can be made to the counselor's learning about his client, there is at least the suggestion in Beam's results that the readiness with which this learning occurs is related to the degree of the counselor's comfort in the counseling relationship.

Studies by Taffel (90), Verplanck (93), and Wickes (94) are comparable to the now-famous study by Greenspoon (36) which suggested that counselor responses might selectively reinforce clients' verbalizations. Taffel (90) presented cards to subjects on which six pronouns and a verb were arranged. The subjects responded to the cards with a sentence. Under the experimental conditions the experimenter responded by saying "good" or by flashing a small light at the end of any sentence that started with "I" or "We." He found that "good" was an effective reinforcer but that light was not; he also found a relationship between scores on the Taylor Manifest Anxiety Scale and amount of conditioning. Verplanck (93) found that reinforcements of statements of opinion by agreement or paraphrase produced an increase in the rate of speaking opinions. Wickes (94) found that frequency of movement percepts elicited by use of achromatic ink blots was significantly influenced by comments of the experimenter such as "good" or "fine" or by actions such as smiling or nodding.

Bieri (9) investigated predictive accuracy and assimilative projection as related to cognitive complexity. Cognitive complexity was ascertained by use of Kelly's Role-Construct Repertory Test, and subjects were asked to

predict the behavior of two classmates on a Situations Questionnaire. Bieri found support for the hypothesis that (a) a positive relationship should be found between degree of cognitive complexity and predictive accuracy and (b) a negative relationship should hold between cognitive complexity and assimilative projection. If counselor sensitivity is operationally defined as predictive accuracy, Bieri's results are relevant to a central aspect of the counselor's role.

In a study investigating counselor participation more directly than those cited above (with the exception of the study by Parloff) Strupp (86) found a demonstrable effect of personal analysis upon therapists' verbal behavior, an effect independent of level of experience. One of his findings was that analyzed therapists tend to be more active, i.e., make a significantly smaller number of silent responses.

Methodological studies.—A wide range of conception as to how the counseling process might be investigated lends itself to an exhaustive report of methodological studies. However, an enumeration of such studies independently of the theoretical rationale associated with methods would not appear to be too fruitful. Studies of some aspect of interviewing, on the other hand, are of general interest, and four such studies are, therefore, reported.

Harway *et al.* (41) employed a seven-point scale to obtain ratings of depth of interpretation. They found that the ratings discriminated between interviews and that variations in unit size (entire interview as unit versus response units within interview) and in certain conditions of context of therapist's statements led to changes in ratings of depth, but, as predicted, that ratings did not differ when based on a typescript as compared to a tape recording of a therapy interview. They present their study as an illustration of a check on validity of measurement by examining the consistency of the measurements with the definition of the variable and conclude that their results provide evidence for validity. In a subsequent article Raush *et al.* (66) report four studies designed to investigate the dimensional characteristics of depth of interpretation. Although evidence was not found for the treatment of depth of interpretation as a unitary dimension, depth of interpretation was consistently the primary dimension.

Saslow, Matarazzo & Guze (74) found the interaction chronograph, "a device which allows an observer to record in time units with a high degree of precision the behavioral interaction of two individuals in terms of some ten or more variables," to be reliable and also found stability in patient interaction patterns to be associated with inter-interviewer consistency, and flexibility in these patterns to be associated with intra-interviewer variance.

Giedt (35) presented interviews as silent films, written transcripts, sound recordings or complete sound films and secured ratings of the personality characteristics and predictions of the responses to incomplete sentences of the patients interviewed. He found significantly greater accuracy of personality ratings as well as better predictions when content cues were included as in the written transcript, sound recording, and complete sound film as compared to the silent film. Giedt also reports that "an

interesting difference from the trend towards gradual improvement with additional cues found with the ratings was seen with the predictions where somewhat poorer predictions were made in the complete sound film condition." He goes on to offer the interesting hypothesis that "visual cues or a patient's appearance may impair predictions."

Studies of outcome.—Cartwright (15) in a provocative paper keeps the "pot boiling" in 1955 over the question as to whether psychotherapy is effective; he claims that the mere absence of evidence of effectiveness under the conditions of a matched-control-group design is not conclusive. He holds that the definition of the statement, "it is demonstrated that psychotherapy is effective," should read:

It is demonstrated that psychotherapy is effective if $X > Y$ (where X is the mean proportion of recovery for experimental groups and Y the mean proportion of recovery for matched control groups) under a matched-control-group design; or, if time in therapy shows a greater rate or degree of recovery than time not in therapy under an own-control design; or, if degree of recovery is found to be a function of the number of interviews under a concomitant variation design, or; if . . .

Cartwright becomes another spokesman, then, on behalf of Eysenck's position. Rosenzweig (72) and DeCharms, Levy & Wertheimer (22) challenged Eysenck (25) earlier when he suggested that patients not receiving psychotherapy improve as much as those who do. While Cartwright's paper is a thoughtful exposition which provides useful perspectives toward questions of evidence, there can be little argument with Shoben's statement (77) that "the conclusive evidence of therapeutic effectiveness must come not from argument but from relevant and vigorous research."

A much-needed perspective toward research evidence of "therapeutic effectiveness" is furnished by the volume on *Psychotherapy and Personality Change* edited by Rogers & Dymond (69). In the chapter concerned with the development of a program of research in psychotherapy, the writers point out that they and their colleagues have abandoned the question as to what constitutes successful psychotherapy in favor of asking, "What are the concomitants of therapy?" Specifically, they say:

We may summarize some of these research plans for measuring outcomes by observing that the chief characteristic is a multi-directional approach to the assessment of personality and behavioral concomitants of the therapeutic process. In the long run we may be able to make a number of valid statements about the concomitants of therapy which will permit an adequate social judgment as to its usefulness.

The multi-directional approach of which these writers speak is embodied in a decision to test hypotheses derived from theory in order that findings might reflect back upon a body of theory broader than the specific hypothesis investigated. Such a procedure is, of course, designed to hasten the day when a tested and "workable" body of theory holding implications for personality functioning may be available as a guide to criteria of "successful psychotherapy." It is of interest in this connection that the planning of the research reported in the Rogers & Dymond volume is not only based upon

implications of personality theory for therapeutic procedures, as advocated by Worchel (103) in a critique of psychotherapy research, but also upon implications for personality organization and functioning. The selection of the Rogers & Dymond book for the annual research award of the American Personnel and Guidance Association would seem to be thoroughly justified not only because of the research contributions as such that are reported in this volume, but also because of its clarification of theoretical issues and of the criterion problem.

Studies of outcome of counseling reported in 1955 cannot be sharply divided in accordance with the extent to which they are colored by considerations of "success" as a criterion. Studies such as those performed by Harris, Firestone & Wagner (40) and Faries (26) employ pragmatic criteria. In the first of these studies recruits with an admitted history of enuresis were assigned to experimental and control groups, the experimental group receiving a brief psychotherapeutic session oriented around the problem of enuresis. A higher proportion of the control group, it was found, were admitted to the psychiatric observation ward prior to completion of recruit training. Faries (26) found that counseled students graduated in significantly larger numbers than noncounseled students with whom they were matched but he acknowledges that personal motivation of the counseled students would appear to be the largest uncontrolled variable. Somewhat in contrast to the two foregoing studies is one by Gibson, Snyder & Ray (34) which was concerned with a factor analysis of 20 measures of change from the interview, Rorschach, and MMPI rather than more pragmatic, or more immediately pragmatic criteria. They interpreted three rotated factors which tended to correspond to the kinds of instruments used rather than a hypothesis concerning a psychologically meaningful structure. The factors, corresponding as they do with the kinds of instruments used, point up the necessity of evaluating therapy with a battery of tests, according to the authors.

The remainder of the outcome studies to be reported can be encumbered in one fashion or another with the "success" question, although they can also, of course, be thought of as studies of concomitants of counseling or psychotherapy without reference to the question as to whether counseling or psychotherapy is effective in some socially-valued way.

Wiener (95) conducted an investigation of two experimental counseling techniques which affords a model of an experimental investigation of counseling designed to answer the question as to what happens under given conditions without involving any value-laden question. He induced stress by interpreting Rorschach protocols to subjects in such a way as to imply possible indications of maladjustment and then divided them into four groups: reassurance-interpretation, catharsis-reflection, talk, and rest. Although the improvement was not always significant on the post-test measures for the counseled groups combined, it was consistent in comparison with the noncounseled groups combined. Neither of the two experimental counseling techniques, reassurance-interpretation or catharsis-reflection, was found to be superior to the other in improving performance.

Barron & Leary (4) offer an important observation pertinent to experimental procedure in a study of changes as ascertained by MMPI scores in psychoneurotic patients with and without psychotherapy. They found no significant difference in improvement between therapy patients and waiting-list controls. This finding suggests that therapeutic factors may operate in the initial intake procedures, and the authors, therefore, suggest that additional "nonwaiting-list controls" might be employed in future research. (The possibility that intake interviews which focus upon "realistic questions" and "possible courses of action," as was the case in this study, might be as "effective" as more prolonged counseling or psychotherapy is of considerable interest in and of itself.)

Rosenthal (71) studied changes in moral values following psychotherapy and found that improved patients revised certain of their moral values to correspond more closely to those of their therapists while the values of unimproved patients became less like those of their therapists. He points out that the findings are inconsistent with Mowrer's notion (56) that "superego functions or values can be influenced very little, if at all, by psychotherapy."

Cartwright (16) found the variables of sex and age were not significantly related to ratings of success in client-centered psychotherapy but that "students were somewhat more successful than non-students." Within each of a group of short-case and long-case clients there was a strong positive relation between number of interviews and success rating. "A 'failure zone' ranging around 17.5 interviews was interpreted as a period during which potentially long-case clients dropped out of therapy."

MEASUREMENT

Self-concept studies.—Several studies appear in 1955 which reflect continuing interest in correlates of the self-concept, an interest which has presumably arisen from the theoretical importance which the Rogerians attach to the self-concept. Studies by Berger (8) and Rosen (70) report relationships between MMPI scores and self-perceptions. Berger (8) found that several MMPI scores correlated significantly with self-acceptance and several with acceptance of others, although there were differences among the scales which correlated significantly for men and women. An interesting feature of Berger's paper is an explanation of the possibility of considering K as "measuring something more than 'defensiveness,'" i.e., something akin to self-acceptance. Rosen's finding (70) that "the more defensive the student, the fewer the items which are not endorsed but regarded as desirable, or endorsed but regarded as undesirable" presents an interesting problem of interpretation in view of Berger's suggestion as to what K measures. Rosen also found that depressiveness, psychasthenic traits, schizoid traits, or introversion hold a relationship converse to that between defensiveness and discrepancy between self-endorsement and desirability, i.e., the higher the standing in these traits, "the more one finds items for which self-endorsement and desirability are discordant." A further aspect of Rosen's results was evidence for a general tendency toward integration or lack of it in rela-

tion to conflict between self-appraisal and personal standards, between self-appraisal and perceived social standards, and between personal standards and social standards.

Friedman (31) had normals, neurotics, and paranoid schizophrenic subjects do a self-sort of 80 statements according to *Q*-sort method. The same statements were sorted by Friedman to characterize the TAT hero of each subject, with a reliability study being performed which demonstrated that other judges could replicate Friedman's sortings. The results indicated realistic positive self-attitudes for the normals, unrealistic positive self-attitudes for the paranoid schizophrenics, and negative self-attitudes based upon realistic perception of disturbance within the self for the neurotics.

Block & Thomas (11) found support for the hypotheses that "(a) degree of self-satisfaction is curvilinearly related to the social dimension of adjustment and (b) degree of self-satisfaction is ordinally related to the conceptual dimension of ego-control." The first hypothesis may bear a relationship to Friedman's finding that normals and paranoid psychotics both exhibited positive self-attitudes.

Cowen, Heilizer & Axelrod (18) carried out an ingenious test of the assumption that self-rating discrepancies are conflict indicators. Using the Bills-Vance-McLean Index of Adjustment and Values with college freshmen as a means of selecting neutral and conflict words, they found elevated learning thresholds "for material associated with words identified as conflictual by virtue of self-rating discrepancy."

Studies of interests.—Two significant contributions to knowledge of vocational interests in 1955 are Strong's *Vocational Interests 18 Years After College* (85) and Darley & Hagenah's *Vocational Interest Measurement* (21). For a group of 663 cases for whom the interval averaged 18 years between test and final retest on the Vocational Interest Blank, Strong found: (a) that "there are 78 chances to 22 that a man with an A rating will enter that specific occupation and 83 to 17 that a man with a C rating will not enter the occupation," (b) "test-retest correlations for scores on 17 scales over an interval of 18 years average .69," and (c) the tetrachoric correlation between a combined satisfaction score and Vocational Interest Blank score for the corresponding occupation was .30. It would appear that Vocational Interest Blank scores tend to predict occupation rather well and that they also show considerable consistency over a period of years. The lower degree to which they will predict satisfaction in an occupation may be attributable, as Strong points out, to the homogeneity of the sample in respect to success as well as the complexity of satisfaction. The comprehensiveness of the book by Darley & Hagenah is suggested by its chapter headings which cover the following subjects: the meaning of work and jobs, the structure of interest measurement, the analysis of interest patterns, personality and interests, the origin and development of interests, and the use of the Strong Vocational Interest Blank in individual cases. This book not only organizes the literature on interests in a meaningful way but also furnishes very useful leads to the counselor in interpreting and presenting interest scores.

McArthur (52) compared job title analysis with Darley's pattern analysis of Strong Vocational Interest Blank scores and found that the latter "did not improve the accuracy of long-term predictions of occupations." He adds that, "The chief value of pattern analysis seemed to be the possibility of saying something meaningful about a testee's chances of following a career for which no Strong scale exists." This value as well as the value of an over-all picture of interest patterns are by no means negligible advantages to the individual counselor, needless to say, and are not necessarily diminished by McArthur's results.

Gehman & Southern (33) obtained evidence indicating that Part A of the Kuder Electrical Engineering Occupational Scale differentiated between freshman electrical engineering students and other freshman engineering students. Part B, which elicited responses when the students were trying to make a "best impression" did not improve the identification of interests of the former. The authors suggest that the inventory items may not be as transparent as might be expected.

Newman (60) and Forer (29) contribute studies of Kuder Preference Record scores as they appear to relate to personal adjustment. Newman's data suggest that high scores of tuberculous patients on the Social Service and Persuasive scales are related to disturbance in social and interpersonal relationships. Forer (29) found a group of emotionally and/or physically disabled veterans significantly lower than the norm group in Mechanical interest and higher in Literary and Social Service interest, although a significant decrement occurs in Social Service and a rise in Musical interest upon retest following occupational failure experiences. Forer also refers to an ongoing study of 26 different nosological groups, 14 of which are significantly elevated and none depressed in Social Service interest, and he says he is prepared to speculate that high Social Service interest sometimes reflects denial of underlying hostility. Newman offers an alternative interpretation in relation to high Social Service scores of tuberculous patients but one that might have broader generality, i.e., such scores reflect a desire to strengthen interpersonal relationships which have been threatened.

In still another study of interests holding significance for personal adjustment Haselkorn (42) administered the Kuder Preference Record and Strong Vocational Interest Blank to homosexuals and two matched control groups and found that significant differences tended to revolve around those scales representing "traditional male" interests rather than those representing "traditional female" interests. It is interesting to speculate over the possibility suggested by these results, i.e., that homosexuality in males may reflect failure in own-sex identification more than "success" in opposite-sex identification. Berger's finding (8) that the more feminine the interests the male shows, the poorer the integration between self-appraisal and personal desirability may bear out this speculation to some degree.

Prediction studies of adjustment.—The MMPI continues to receive attention in 1955 as a predictor of adjustment with note being taken by Black

(10) of its growing use with normal persons. He presents data to indicate that local and regional norms are unnecessary, discusses the importance of profile analysis, and summarizes a study demonstrating the effectiveness of the MMPI in differentiating between groups of college women in relation to personality and adjustment differences. In another study of college women Osborne, Sanders & Young (62) found that college disciplinary offenders could not be identified by elevated scores on the Pd Scale, that nonoffenders are significantly more masculine in their interest patterns, and that scores on the Pa, Pt, Sc, and Ma Scales were significantly higher for the problem group.

In a "further validation" of Barron's Ego-Strength Scale, derived from the MMPI, Wirt (101) found that the scale as a whole separated "greatly improved" from "unimproved" patients treated by psychotherapy, although only one of the individual items separated these groups significantly.

Papers by Stotsky & Weinberg (83), Crandall & Bellugi (19), and Fricke (30) report upon varying techniques having significance for the prediction of adjustment. In the first of these studies the authors were concerned with the prediction of psychiatric patients' work adjustment. They constructed work-relevant sentence stems pertinent to nine ego-strength dimensions and found that eight of the nine variables were significantly related to successful performance in manual arts and educational therapy. "Self-reliance" was the best predictor of treatment outcome, however. Crandall & Bellugi (19) investigated Social Reinforcement Index ratings, the scoring units of which were derived from Rotter's social learning theory and pertain to rewarding and nonrewarding social experiences. Social Reinforcement Index scores were compared with ratings of freedom of movement, one of Rotter's constructs having to do with expectancy of obtaining positive satisfactions. High correlations were obtained. Fricke (30) proposes a configural-content-intensity item consisting of paired statements to which the test taker is asked to indicate "true" in one column and "false" in another, and then to indicate in a third column the answer with which he has the stronger agreement. Fricke speaks of the third response as "a 'built-in' forced-choice situation on the intensity dimension," discusses the discriminating capacity of the configural-content-intensity item, and suggests that it "might be sufficiently sensitive to minor differences in personality to permit the measurement of currently unmeasured, or unsatisfactorily measured personality dimensions."

Stotsky (81) and Neff (59) conducted studies relevant to the prediction of occupational success. Stotsky found three biographical items (arrests for any except psychiatric reasons, difficulty with alcohol prior to the past two years, and present difficulties with alcohol) differentiated between successful and unsuccessful on a hospital member-employee rehabilitation program. He adds that, "Two main reasons for failure on the program were exacerbation of psychotic symptoms and over-indulgence in alcohol," and that, "From these findings it appears that alcoholics and incompletely stabilized psychotics are poor prospects for a program of this type." Neff (59) found that Rorschachs of persons who profited from occupational work-shop training

tended to improve while those of subjects who did not profit tended to worsen, but he did not find the Rorschach to be an efficient predictor of vocational rehabilitation.

Kvaraceus (46) not only lists seven instruments considered as the most promising now available to research workers concerned with identification of potential delinquents but also goes into a broader consideration of test construction. He discusses problems of defining criterion groups and restricting interpretation of data to the behavioral phenomenon making up the validation sample, and he also suggests that the "test-maker may need to seek out approaches that are more in line with multi-variate analysis" rather than "giving primacy to certain variables to the exclusion of all other information."

A broad issue pertaining to prediction of adjustment status is set forth by Tindall (92) who advocates use of several indices of adjustment in evaluative studies in which claims are made in regard to change in adjustment status. Before accepting such a seemingly reasonable recommendation too wholeheartedly, it must be recognized that the indices of adjustment or change employed depend upon the purposes of the investigator. If he is predicting particular changes derived from theory, then some indices of change would be pertinent to his purposes and others would not, and the use of several indices, advocated by Tindall, would not necessarily be in order. It is also conceivable that such theoretically-derived predictions might be made to measures supported by criteria of internal consistency rather than by external criteria. Thurstone (91) asserts that "the old test-criterion correlation is an obsolete formulation" and that "we shall reach the stage of sophistication in dealing with personality tests where the primary factors will undoubtedly turn out to be some dynamic characteristics of temperament." The prediction of changes in such characteristics of temperament, or other internally-consistent measures, is a defensible enterprise regardless of the outside criteria to which such changes might be related. The investigation of change in "adjustment status" need not necessarily wait, then, upon determination of the relationship of measures of change to outside criteria.

VOCATIONAL AND REHABILITATION COUNSELING

Two approaches to advancing knowledge of vocational guidance and choice are reflected in important papers by Froehlich (32) and Meadow (54). Meadow presents as a major thesis the proposition that "a meaningful theoretical formulation is a necessary accompaniment to further significant progress in vocational guidance counseling." Froehlich, on the other hand, pleads for attention to ultimate criteria and says that "no immediate criterion has meaning except in relation to ultimate criteria." ("The number of students participating in a career day may be an immediate criterion of its effectiveness. . . . But the persons who planned the career day probably would not have done so if they did not either implicitly or explicitly think that the career conference would contribute to future vocational adjustment

of the participants or to some other condition which we might label an ultimate criterion.")

While the spokespersonship of Meadow and of Froehlich are not necessarily antagonistic, the emphases of their messages do differ. Although Meadow concerns himself primarily with the problem of vocational choice, implicit in his stress on theory is the idea that the outcomes of vocational counseling can be deduced from logical constructions of the nature of the process. This view allows for the selection of criteria that are relevant to the deductions whether they be "immediate" or "ultimate." Froehlich's stress on ultimate criteria, on the other hand, tends to result in the view that immediate criteria can be adopted only by default when ultimate criteria are inaccessible. The papers by Meadow and Froehlich point up a question of major significance to counselors, vocational or otherwise.

An exploration of the literature pertaining to rehabilitation counseling for 1955 generates large visions of man's expanding potentials. While this literature applies specifically to rehabilitation of the physically or emotionally disabled, the emerging concepts summon a view of men as self-expressive entities capable not only of "getting well" but also capable of "flowering." Stubbins & Napoli (87), for example, speak of "the conviction that the mentally ill person can obtain satisfactions from appropriately selected work experiences which will spread and enrich the whole of life." Cohen (17) in similar vein says, "That the extramural life situation of the psychotic may be of crucial dynamic significance to his pathology has only recently received theoretical recognition." If reference to pathology, psychosis, and mental illness were removed from the foregoing quotations, they might be combined into a proposition to the effect that the enrichment and expression of man's resources are "nourished" or "stunted" by the nature of his life milieu and the use he makes of this milieu. In the light of a proposition of this kind, the counselor's function can readily be envisioned as one that rests upon a positive conception of expanding realization of personal resources rather than the more limited concept of recovery. Perhaps the counselor of the future will be viewed in somewhat different perspective than the rehabilitation counselor, clinical counselor, or even the vocational counselor of today. Rehabilitation, psychotherapy, and vocational guidance may come to be regarded as important but subordinate and partial aspects of "human-resources counseling." This human-resources theme, as has been suggested, is implicit in the papers by Stubbins & Napoli and Cohen, both of which are concerned with vocational planning in relation to mental illness. This theme can also be discerned in somewhat similar papers by Needleman (58) and Stotsky (82) who emphasize that an adequate program of treatment makes provision for the reintegration of the neuropsychiatric patient into the community.

Stotsky, Daston & Vardack (84) also based an experimental study upon a hypothesis consistent with the human resources theme, i.e., that "the counselor, by instituting concrete vocational planning, might encourage pa-

tients to progress toward ultimate discharge," and obtained results in support of this hypothesis. In still another study consistent with the human resources theme, Feintuch (27) found that an integrated program of vocational counseling, casework, and a sheltered workshop increased employability and favorably modified attitudes associated with employability of difficult-to-place persons.

Several excellent papers have appeared in 1955 having to do with the orientation, training, and methods of the rehabilitation counselor. Block (12), for example, sets forth three "operational principles for counseling the disabled" embracing the psychological and social-psychological ramifications of disability, the depth process in counseling, and the holistic or team approach. "These principles require the counseling psychologist to counsel the whole person . . .," Block states, and he goes on to point out their implications for training.

A concluding "overview" presented by DiMichael (23) in an excellent paper on "Applicability of Standard Psychological Tests to the Disabled" is well worth reproducing as a guide to psychological evaluation of the handicapped. DiMichael says:

The psychological evaluation of the handicapped calls for considerable flexibility in approach, experience with the various handicapped groups, and primary dependence upon clinical judgment. For the most part, the psychologist must use the test standardized on the nondisabled. This may require modifications of test procedures, changing of time limits, selection of certain parts of tests and omissions of other parts. The interpretation of test results must take into account the special features of the disability, both organic and psychological. It also is necessary to consider the age at onset of disability, to appreciate that a period of some instability is found immediately after the disability is incurred, and that handicaps acquired in adulthood are deeply influenced by personality patterns antedating. A test-centered approach is fallaciously scientific although it may be justified in some kinds of research.

Mase (50) stresses the psychologist's responsibility for interpreting and explaining his test data to co-workers who can direct their efforts "to much more realistic goals if they have at their disposal the findings of the psychologist." He stresses the importance of active educational efforts on the part of the psychologist to counteract fears he may have of his findings being misused. Mase's thinking agrees completely with that of Sewall (75) who stresses the need for the team approach in counseling with the disabled and says that "What happens when responsibility is not shared is that each specialist jealously guards the prerogatives of his field. The patient is then neatly sliced into isolated sections."

Just as DiMichael's paper furnishes an excellent guide to psychological evaluation of the handicapped, Kutash's paper (45) on "The Application of Therapeutic Procedures to the Disabled" provides a competent and comprehensive summary of the various facets of psychotherapy with the disabled. While emphasizing that "There is no special therapeutic approach for each type of disability," he points out that "there are certain nuances and

emphases . . . that are peculiar to the disabled group." He then reviews these nuances in relation to referral for therapy, transference, reorientation, interpretation, and group therapy.

In view of his three principles, discussed above, which embrace the psychological and social psychological ramifications of disability, the depth process in counseling, and the holistic or team approach, Block (12) asserts that the training of the rehabilitation counselor should provide working knowledge of both clinical and counseling psychology and practicum training in a medical setting. Patterson (64) holds that rehabilitation counselors of the emotionally disabled "should have considerable experience with the emotionally disturbed, similar to that possessed by the clinical psychologist." A statement by Miller, Garrett & Stewart (55) concerning the policy of the Office of Vocational Rehabilitation, while not necessarily in conflict with Block's and Patterson's, leaves the training issue "wide open":

Whether the heaviest weight in terms of course work would be in departments of psychology, schools of social work, departments of special education, departments of counselor training, or other departments, has not been determined and will not be by the Office of Vocational Rehabilitation. It may well be that, in the near future, some intensive study of this problem will indicate that one or another of these departments should take the primary responsibility and that some formal system of accreditation should and will be organized. Until that time, it is the policy of the Office of Vocational Rehabilitation that the organization of curricula for the training of rehabilitation counselors should be determined by each training institution in terms of its available staff, resources, and pattern of organization.

LOOKING FORWARD

A dawning construction of man as a self-expressive entity emerges in 1955 which prompts the prediction, stated earlier, that the counselor of the future will be viewed in different perspective than the counselor of the present. Sanford's concept (73) of creative health summons a vision of man's realization of personal resources which is strengthened by Kelly's view (44) of man's conceptual freedom and Murphy's view (57) of man's capacity for enrichment through participation in his life milieu. Super's stress (89) on building on the individual's assets and Foley's reference (28) to circumventing implications of weakness or personal imbalance would seem to reflect something more than isolated instances of positive perspectives. If so, society's perception of the counselor's role, as well as his own perception of it, may be shifting toward a broader interpretation of his function as a "human-resources consultant." As such, his interest in self-expressive styles of living might be expected to encompass a somewhat more limited kind of interest in pathology and rehabilitation as well as in vocational and educational choice. The brand of knowledge sought might at the same time be expected to revolve increasingly around the question of the nature of creative health, a question emerging, it would seem, from the construction of man as a self-expressive entity.

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STATISTICAL METHODS¹

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INTRODUCTION

The increase in the amount of literature pertinent to quantitative methodology in psychology which was emphasized in previous reviews is continuing. An attempt has been made herein to offer extensive coverage, although often at the expense of deleting desirable summary or evaluative material initially intended for inclusion. Hence the reader will, in most cases, have to refer to the original sources for details. It is the hope of the reviewer that enough interpretative material has been included to enable the reader to make a reasonably good judgment as to the value of his pursuing any particular reference further.

ANALYSIS OF VARIANCE

A thorough and systematic study of various assumptions made and of the bias introduced by ignoring them in the various two-way classification analysis of variance designs was undertaken in a number of papers (36, 37, 60, 101, 117, 118, 123). The necessity of selecting a mathematical model that is appropriate to the experimental life situation and the important role of the assumption of additivity when using a linear model were emphasized by all.

Kempthorne (60) points out the lack of distinction made by many statisticians between analyses based on randomization theory and the normal law model (normally distributed errors) and discusses a number of confusions and conflicting outcomes which exist in the literature because of this lack of differentiation. He considers randomization theory to be more basic and presents randomization analyses based upon a linear model for completely randomized designs, complete randomized blocks, modification of randomized blocks, Latin square, and the Knut Vik square. The loose way in which the term additivity has been used is criticized, and it is defined in this paper as absence of interaction between treatment and ultimate experimental unit. He says that tests of homogeneity of variance tend to be used somewhat as tests for additivity and makes the point that the main concern of the experimenter should be toward the determination and use of a scale for which treatments are additive in their effects. Greater use of Tukey's test for additivity is proposed. In spite of these criticisms, Kempthorne offers a certain amount of comfort to the many users of analysis of variance by concluding, "It should be realized that the analysis of variance test with the

¹ The survey of the literature pertaining to this review was completed in May, 1956.

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F distribution has a fair basis apart from normal law theory and is probably in most cases a good approximation to the randomization analysis of variance test."

Wilk (117) presents a randomization analysis of a generalized block design, using a somewhat weaker definition of additivity, namely, that the unit-treatment interactions are zero within blocks but not between blocks. The expectations of the mean squares under this randomization procedure are compared with their normal theory analogues, and it is shown that randomization tests in many situations may be approximated by tests based on normal theory.

Wilk & Kempthorne (118) stress that rules and results, concerning expectations of mean squares and the choice of error terms, are given by one group of statisticians and contradicted by others largely because explicit and objective methods for obtaining an appropriate model consistent with natural phenomena of a given experiment are not generally used. A generally applicable method for the derivation of a linear statistical model, based on the experimental situation and randomization theory, is described and then applied to the two-factor problem in analysis of variance. Error terms to test various hypotheses are derived and their appropriateness examined. The authors advocate adopting the fundamental notion of randomization tests and using normal theory as approximations to randomization distributions.

Scheffé (101) is concerned with using somewhat less restrictive assumptions for the problem than Kempthorne. His basic development does not assume that fixed row effects are independent. The basic assumption on the rectangular array of the $\{m_{ij}\}$ is that the columns are distributed independently like a vector random variable. Sometimes in his development an assumption of normality is added and sometimes one of symmetry is utilized. The expectations of the derived mean squares turn out to have the same values as those found from certain more restrictive models. Although certain familiar tests and confidence intervals are justified, some aspects of his results appear to be novel. The over-all test H_A given for the fixed main effects and the associated multiple comparison method previously developed by Scheffé require Hotelling's T^2 . Scheffé shows that the use of the usual F -test in this situation is unjustified.

Gourlay (36, 37), using models and variance component procedures similar to those of Wilk, considers possible F -test bias for educational and psychological experimental designs which may be considered derivatives of the agricultural randomized blocks and Latin square designs. Fixed variate, random, and mixed models (36) are considered for the two-way classification problem. The procedures utilize a B -ratio index obtained by taking the mathematical model applying to the experimental situation and deriving analytically the expected values of the variances involved in the F -test. The ratio of the expected value of the "treatments" variance to that of the "error" variance defines the B -ratio.

Three possible sources of F -bias [heterogeneity of variance within subclasses (from row to row), heterogeneity of variance within columns and heterogeneity of correlation between β -effects (of one column with another)] are considered in each of three situations: equal frequencies, proportionate frequencies, and disproportionate frequencies in subclasses.

Positive bias (a larger number of significant F 's) is demonstrated for many of these situations. Using the same method Gourlay (37) studied F -test bias for various designs of the Latin square type. He, also, considers the appropriateness of F -tests for various Latin square designs by variance component analysis. Certain conditions are described in which a Latin square design is appropriate without zero interactions. In many situations the F -test may give either a smaller or larger proportion of significant F -ratios than is warranted by the F -distribution. As has been reported by others, specific single Latin square designs are shown to be of little use when interactions are present.

The use of the single $k \times k$ Latin square as an approximation to a factorial design is also discussed by Youden & Hunter (123), who propose the addition of k experiments so that each row, column, and letter enters into $k+1$ measurements. Such a design permits a test as to whether the requirement of additive effects for rows, columns, and treatments has been met.

A number of articles on special experimental designs were presented. Three papers (11, 104, 111) considered various aspects of the incomplete block design. Thompson (111) discusses the relative size of the inter- and intra-block error and considers linked block designs. Bose & Clatworthy (11) consider the theoretical properties of partially balanced incomplete block designs. Theorems, corollaries, and lemmas describing the properties of such designs are given. Sprott (104) studied the balanced incomplete block design. Various methods of constructing such designs and the impossibility of obtaining certain symmetric designs are demonstrated. The relationship between balanced incomplete block designs and tactical configurations of the Steiner system is presented.

Experiments utilizing data which are nonorthogonal often cause difficulties. Ditchburne (26) describes and illustrates a method of analysis suitable for use in a double classification problem in which the data are in a triangular table. The triangular type of design is useful in experiments in which levels of one factor may be found from the sum or difference of two other factors. Another source of nonorthogonality is the occurrence of unequal numbers of observations falling in the various subclasses. Kramer (61) presents a method for a two-way classification with unequal subclass numbers which is less involved than the familiar procedures. It is useful, however, only if interactions are absent or negligible.

A number of papers were concerned with making statistical decisions after estimating variances or testing hypotheses by F -tests. Welch (116) discusses the general problem of estimating a quantity, V , which is a linear function of population variances. He also considers its error and confidence

limits using several approaches such as the assumption of normality and Pearson type III curves. One of his approaches resulted in an expression for confidence limits of V which was independently obtained by Huitson (53). Huitson also presents tables for estimating the confidence intervals for the sum of two variances.

McHugh & Ellis (78) and Dunnett (27) considered the problem of making contrasts between specific means among a group of mean values. McHugh & Ellis (78) describe an application of Scheffé's procedure which permits comparisons suggested by the data. The process essentially involves altering the size of the critical region. Dunnett (27) presents a procedure for comparing a number of treatments with a control and making a statement that the probability of all confidence statements being correct will be equal to or greater than a specified value. In view of the fact that only comparisons involving the control are of interest such confidence intervals are smaller than Scheffé's or Tukey's. Tables are presented.

Ramachandran (96) derives simultaneous confidence bounds for two different parameters. One involves intervals for all ratios of variances of concern in the experiment and the other intervals for the means of the main effects. Ghosh (35) making use of the procedures of Scheffé and Tukey presents procedures for setting up tests of significance for groups of parameters which are correlated in various ways.

There were a few interesting techniques that might be useful in teaching analysis of variance. Schultz (102) presents rules of thumb for determining expectations of mean squares in analysis of variance. Li (66) and Barnes, Pearson & Reiss (9) describe graphical ways of illustrating sums of squares and the relationships between the partitioned sums of squares.

Grant (39), and Outhwaite & Rutherford (92) consider the problem of using curvilinear relations in the analysis of variance. Grant presents an illustration of the use of orthogonal polynomials to test the adequacy of functions of varying degrees to describe a systematic trend in learning data. Outhwaite & Rutherford (92) show how to use covariance analysis to eliminate linear, quadratic, and higher degree trends in a randomized block design. They report that "Even after the full covariance adjustment (up to the sixth degree) the design used turns out to be less efficient than a 7×7 Latin square with the same variance per plot."

As may be noted, there has been considerable concern during the past year about the properties of different models and in particular the extent to which failure to meet specific assumptions introduces bias in F -tests. Topics such as reliability utilizing analysis of variance are included in other sections of this review.

DECISION THEORY

Increasing interest in and application of decision theory is observable throughout the statistical literature. Examination of familiar statistical tests in terms of decision theory and the proof of mathematical theorems,

highly theoretical in nature and having little immediate practical value for the psychologist, have been frequent during the past few years. However there is an increasing tendency to consider psychological questions in terms which permit utilization of decision theory.

In view of the excellent description and discussion of the decision problem by Moses (86), only an enumeration of some mathematical contributions during the current year will be presented, although the impact of decision theory is apparent in articles included in other sections of this review. A large number of statistical problems are reduced by decision theory to the selection of a distribution function on the basis of N observations. The problem is to select a class $\{\delta\}$ of decision functions and to arrive eventually at a decision δ_{nj} . A number of articles have appeared recently which attempt to deal with this problem.

Laderman (62) studied the asymptotic behavior of decision procedures for a class of multiple decision problems in which a random variable, X , is known to have a distribution function belonging to the distribution space $\Omega = \{F_i(X)\}$, $i=1, 2, \dots, k$, and it is desired to select the true distribution function based on a sample of N independent observations of X . Asymptotically minimax sequences of decision procedures and asymptotic admissibility are examined. A detailed study is made of the class of problems where Ω consists of k univariate normal distributions having the same variance but different means.

Aggarwal (1) presents some minimax invariant procedures, which are essentially step functions, for estimating a cumulative distribution function of a one-dimensional random variable for various loss functions. Two classes of loss functions are considered, and a numerical illustration is given. To apply the tests, tables of the incomplete beta-functions or a special table given by Aggarwal are used.

Kac, Kiefer & Wolfowitz (57) consider the problem of goodness of fit using functions based on distance. It is shown that the asymptotic power of these tests is considerably greater than that of the optimum χ^2 test. Two tests of normality are presented, their use is illustrated numerically, and tables of the distribution functions are included. The problem of testing goodness of fit by distance functions is considered also by Matusita (77). Distance is given as a Lebesgue integral defined on an arbitrary set. He establishes a simple upper bound on the risk and shows how to make the risk smaller than any preassigned value ϵ .

It is well known that conventional tests such as the F -test in analysis of variance do not supply all the information the experimenter seeks. In most cases the experimenter actually wants a decision procedure for selecting populations or a population possessing a desired characteristic, e.g., the population with the largest mean. This problem has been considered by Tukey, Scheffé, and others. Seal (103) presents an infinite class of decision rules for choosing a group of populations from a given set of normal populations which should contain the population with the largest mean. The

problem of selecting an optimum rule from this class is also studied, and a rule which is similar to the t -test is given.

CORRELATION, REGRESSION AND CLASSIFICATION

An argument of long standing among psychologists is the relative merits and the appropriate use of biserial $r(r_b)$ and point biserial $r(r_{pb})$. Tate (107, 108) in two very systematic, comprehensive, and rigorous discussions examines this problem in great detail. Among the numerous points made is the lack of large-sample efficiency of r_b as the population correlation ρ departs from zero. A maximum likelihood estimate ($\hat{\rho}$) is derived and an iterative procedure for computing it is given.

After discussing the question of when to use r_b and r_{pb} , Tate concludes that if there is any doubt concerning the validity of the biserial model, use r_{pb} . On the other hand, if the biserial model is deemed to be valid, then use r_b to test hypotheses of the form $\rho = 0$, and use $\hat{\rho}$ for other purposes. Tate's conclusion is in general consistent with one which Kelley (59) and the reviewer advocate. However, in view of the kind of situation where r_b is most commonly used, the additional labor of computing $\hat{\rho}$ would seem to be seldom warranted.

A problem of great concern to psychologists is the situation where the assumption of uncorrelated errors is not tenable. This condition may occur when nearly any of the usual tests of significance is used. Box (12) has considered this issue for analysis of variance. In the analysis of ordered observations, the errors are often serially correlated. In mathematically complex papers, Hannan (48, 49) and Watson (115) discuss this issue. Hannan presents an exact test for the serial correlation in the residuals from regression, when the residual is a simple Gaussian Markoff process. He also derives an estimator of the regression coefficient whose distributional properties are the same as those of the classical estimates of the regression coefficient. Watson (115) examined a regression analysis making the assumption that the error covariance matrix is $\sigma^2\gamma$ when in fact, it is actually some other value, $\sigma^2\alpha$. When the errors are independently distributed with the same normal distribution, the optimal test statistics for hypotheses concerning regression coefficients have t , or F , distributions. Bounding distributions, which converged to the familiar tabulated distributions as $\alpha \rightarrow I$, were obtained by Watson for these statistics when the covariance matrix of the errors was $\sigma^2\alpha$.

Several articles were concerned with the general problems of classification. Brogden (13) shows through an algebraic development that criterion estimates derived by the usual multiple regression procedures are optimal for personnel classification. Horst (52) presents a procedure to select from a set of predictors a subset of a given size which will give the maximum index of differential prediction among criterion variables.

The recent interest in the problem of assessing similarity of profiles is continuing. An interesting contribution is made by Harris (50) who examines

analogues of Pearson's coefficient of racial likeness and Mahalanobis' distance measure as descriptive measures of profile similarity. A very fine elementary discussion of Euclidean Distance and its matrix representation is given, along with illustrative numerical material. Lykken (75) presents arguments against the use of distance functions in profile analysis.

In a highly theoretical paper Williams (119) reviews and synthesizes the approaches underlying currently available tests of significance for discriminant functions and extends this work to show how such results may be interpreted in terms of multiple linear regression. Exact tests for an assumed linear relationship among variables are developed in terms of an analysis of variance model. The most important practical results are that tests previously given for the adequacy of a hypothetical discriminant function and the new tests given for a functional relationship can be reduced to tests derived by a covariance analysis of the data. The computations are simplified because the test functions can be expressed either as ordinary adjusted sums of squares or as the ratios of determinants of sums of squares and products without determining the latent roots of a matrix equation.

A well-known problem is the fact that correlation tends to exist between obtained gains and initial scores. Garside (34) derives formulas for estimating regression of gains on initial scores which he believes are superior to present procedures.

As may be seen, there have been no highly dramatic developments in this area during the last year. However, the gains are substantial and provide a sounder basis for many of the kinds of analysis psychologists favor.

CHI SQUARE

After the large number of excellent articles on χ^2 which were reviewed last year, it is not surprising to find fewer contributions in this year's literature. Yates (121, 122) discusses the old problem of the appropriate procedure to give an over-all test of significance for a set of 2×2 contingency tables. He examines the combination of probabilities test and compares it with other techniques. This procedure, as is well known, requires the calculation of a significance level p for each of the k tables separately. The test criterion $\sum(-2 \log_e p)$ is then compared with the χ^2 distribution with $2k$ degrees of freedom. Yates concludes that " χ^2 without correction for continuity will give one-tail probabilities for 2×2 tables which may be safely combined in most cases likely to be met with in practice." He indicates that this test is not very efficient and prefers analysis by the method of maximum likelihood, using some appropriate transformation. The detailed methods of this analysis are described (121).

Armsen (5) presents a set of tables for significance tests of 2×2 contingency tables. These tables are based upon the use of the exact hypergeometric formula given by Fisher.

Although a test exists for the more stringent hypothesis of homogeneity of marginal distributions for two completely independent samples, it does

not meet the conditions of essentially bivariate situations where nonindependence of the marginal distributions is characteristic. Stuart (105) presents such a test for homogeneity of marginal distributions in a two-way $m \times m$ classification. He derives the statistic

$$Q = \sum_{i,j=1}^{m-1} V^{ij} d_i d_j$$

whose distribution approaches the χ^2 distribution with $(m-1)$ degrees of freedom. Defining n_{ij} as the frequency in the i th row and j th column, \hat{V}^{ij} is the inverse of the matrix whose elements are $\hat{V}_{ii} = n_{i.} + n_{.i} - 2n_{ii}$, $\hat{V}_{ij} = -(n_{ij} + n_{ji})$, and $d_i = n_{i.} - n_{.i}$.

Frequently one encounters data consisting of a series of proportions, occurring in groups which fall in some natural order. The question of concern is not so much whether any two of the proportions differ significantly, but whether as a group they show a significant trend. Armitage (4) presents a χ^2 test for such a hypothesis. A few months earlier Cochran (19) in a comprehensive article on χ^2 discussed the same test. Cochran also discussed ways of strengthening χ^2 tests when it is possible to predict the type of alternative hypothesis that is most likely to hold if the null hypothesis fails. As an additional proposal he (20) suggests using a linear function of the deviations between observed and expected number, i.e., $L = \sum g_i (f_i - m_i)$ where g_i are numbers chosen in advance such that L will be sensitive to the alternative hypothesis thought most likely to hold. An approximate test of significance of L is given for many different types of situations such as applications to binomial, Poisson, and normal distributions. L can be tested either by treating L/S_L as a normal deviate or $\chi^2_L = L^2/\hat{V}_L$ with 1 degree of freedom. Of special interest is the situation where the g_i 's are determined from an inspection of the data. Cochran shows that "A test that takes account of this selection and errs on the safe side, i.e., gives in general too few significant results, is obtained by referring L^2/\hat{V}_L to the χ^2 table with the number of degrees of freedom used in the goodness of fit test." Conceptually this test is similar to Scheffé's (100) test of any linear combination of treatment means in analysis of variance.

Progress continues to be made in investigating situations to which χ^2 tests have been applied and in devising more adequate techniques.

NONPARAMETRIC METHODS

Developments in this field include new procedures, such as "A Distribution-Free Test of Analysis of Variance Hypotheses" (120), tables to facilitate the use of nonparametric techniques, and additional information about certain well-known tests. Fix & Hodges (31) extend the usefulness of the Wilcoxon test by presenting tables from which exact values of the Wilcoxon distribution may be obtained when the smaller sample size m does not exceed 12. To extend the coverage to larger samples, formulas for estimating these values are also derived. The accuracy of these approximations were investigated and reported to be very satisfactory.

Some interesting and useful "quick sign tests for trend in location and dispersion" are given by Cox & Stuart (22). They are set up to test the hypothesis that N independent random variables have the same continuous distribution function. The various statistics take the form $S = \sum w_{ij} h_{ij}$ where there are N observations and for $i < j$ the score is

$$h_{ij} = \begin{cases} +1 & \text{if } y_i > y_j, \\ 0 & \text{if } y_i < y_j. \end{cases}$$

Some of these tests are shown to be simplified versions of Spearman's and Kendall's rank correlation tests. A sign test is given for trend in dispersion, and a brief discussion is presented on sequential sign tests. Numerical illustrations are shown. The efficiency of these tests is investigated and compared with tests based on the maximum-likelihood estimator. Putter (95) discusses the problems involved in dealing with ties and recommends a nonrandom model.

Four nonparametric tests are compared empirically by Epstein (29) in testing the composite hypothesis $H_0: \mu_1 = \mu_2$ against the alternative hypothesis $H_1: \mu_1 \neq \mu_2$ for two normal populations $N(\mu_1, \sigma^2)$ and $N(\mu_2, \sigma^2)$ of equal variance when the data from each of the two samples become available in an ordered way. The rank sum test was reported as having the best power.

Two papers bearing on Kendall's rank order statistics were examined. Taylor (109) points out that he had presented the procedure for adapting Kendall's coefficient of concordance W to sociometric-type ranking prior to Willerman and gave a formula which is more useful when ties in ranks are encountered. Haberman (47) discusses partially ordered systems and presents tables giving the distribution of Kendall's tau based on a partially ordered system.

Occasionally psychologists deal with problems where either the experimental variable is an angle or where some variable, such as time, has been transformed into angular measure. Greenwood & Durand (41) consider a set of n points ζ_n situated on a unit circle and assumed to constitute a sample from a distribution having *p.d.f.* $g(\zeta)$, where $0 \leq \zeta < 2\pi$. The n random unit vectors thus defined have components $\sin \zeta_n$ and $\cos \zeta_n$ and $V = \sum \cos \zeta_n$, $W = \sum \sin \zeta_n$, and $R = \sqrt{V^2 + W^2}$. The authors show that the statistics V and R provide tests of the uniform distribution $g(\zeta) = 1/2\pi$. Tables of the distribution of R are given by the authors, and the distribution of V was derived.

In a nontechnical article, Wilson (120) using the familiar and much used additive property of χ^2 , illustrates the partitioning of a chi-square statistic for a contingency table into components in much the same manner as a total sum-of-squares is decomposed in analysis of variance computations. The use of this type of analysis is illustrated by making a nonparametric test of hypotheses concerning main effects and interaction ordinarily tested by analysis of variance. This technique is likely to be useful to the psychologist when no suitable normally distributed variable (either direct measurement or transformed) is available.

In concluding this section, the reviewer would like to emphasize a point of view believed important. Considering the numerous admonitions to use nonparametric methods and the enthusiasm with which some psychologists accept nonparametric methods as "the only technique which is really appropriate," it seems desirable to utter a word of caution. Both parametric and nonparametric methods have their place. The basic issue is not one of evaluating the efficiency of a nonparametric method in terms of the corresponding method which would be applicable if the variables were normally distributed, but whether the statistic is appropriate for the data and the problem at hand. Nonparametric methods make use of data which have only ordinal properties. If it is possible to obtain data which have more than ordinal properties, information is disregarded by using rank methods.

TEST THEORY AND SCALING

Over the years, the problem of reliability has occupied an important place in the psychological literature. Burt (15) and Mahmoud (76) in two lucid articles thoroughly review the problem of reliability in terms of analysis of variance. Mahmoud (76) also discusses test reliability in terms of factor analysis theory.

Two interesting papers, one by Lord (68) and the other by Guttman (45), present derivations of formulas for reliability based on assumptions somewhat different than the usual ones. In each study, one of the Kuder-Richardson formulas is shown to be a special case of the newly developed work. Lord considers the problem of reliability in terms of reliability coefficients derived from two definitions of parallel tests. Tests whose items may be considered to have been drawn at random from some large pool of items are called randomly parallel tests. Making only the assumptions: (a) that relationships are restricted to the correlation between randomly parallel tests, (b) that an efficient unbiased estimate of the unknown true variance error of measurement is used, and (c) that the number of examinees is large, he derives a formula which is mathematically identical with Kuder-Richardson formula 21. This derivation is of special interest in that previous derivations of K-R formula 21 required the stringent assumption that all items were equal in difficulty.

The objection of most psychometricians to a coefficient based upon Lord's assumptions, namely that in actual practice tests are rarely produced by drawing items from a pool at random, is considered by Lord who proposes as an alternative a definition of parallel tests based on a stratified sampling of items. The results of his derivation based upon this second definition of parallel tests leads to a formula for the least upper bound of the test reliability.

Guttman (45) derives new formulas to give lower bounds to the reliability of a test, whether or not all respondents attempt all items. The formulas are applicable to completed tests, pure speed tests, pure power tests, and mixtures of speed and power. They are reported to hold both in the sense of

retest reliability and of parallel tests. Various formulas for lower bounds are described and procedures are suggested for obtaining better lower bounds than those reported. In some instances, especially in speeded tests, as Guttman points out, the bounds may be so low as to be useless. One formula reduces to K-R formula 20 under the condition that all subjects attempt all items in the test.

Noble (89) differentiates between test reliability as usually defined and scale reliability where the investigator is interested in the consistency with which successive random samples of judges respond to an invariant set of stimulus items. He shows that scale reliability defined in terms of product moment correlations among successive random samples of ratings by judges is equivalent to the Spearman-Brown formula. Cartwright (16) presents a rapid nonparametric estimate of multi-judge reliability as a function of the number of agreements.

One aspect of variability which is usually treated as unreliability, namely the difference between two responses of an individual at two points in time under the conditions of exposure to the same stimulus and same total situation, was investigated systematically by Mitra & Fiske (84).

Michael & Perry (81) developed a number of formulas for relating item parameters to the mean, variance, reliability, and validity of scores on a power test wherein each item was scored in one of three categories instead of the usual dichotomous situation. Cronbach & Merwin (23) developed a promising model for studying the validity of multiple-choice items. Coombs, Milholland & Womer (21) reported an intensive study of an experimental response method for multiple-choice items which involved obtaining and assessing partial information. Lord (70) discussed the "attenuation paradox in test theory" and showed that with very high item intercorrelations, the relationships between test score and the "common factor" of the items is curvilinear. He stressed the discriminating power of the test at various levels as a more basic concept than validity.

Burros (14) and Rimoldi & Hormaeche (99) independently derived the same formula for the estimation of the discriminial dispersion in the method of successive intervals from different sets of postulates. This formula is $\sigma_j = n/V_j \sum_j (1/V_j)$ where σ_j is the discriminial dispersion for the j th stimulus, and V_j is the standard deviation of the normal deviates for stimulus j . As can be seen, it involves the harmonic mean of the V_j 's rather than the arithmetic mean as used by others, such as Attneave (6). Empirical data presented in both papers give some evidence of the adequacy of the procedure.

Multidimensional scaling offers a powerful scaling method especially in psychological areas of unknown dimensionality. Also, of course, it can be considered to be the general method of which the many unidimensional procedures are special cases. Messick (79) presents an excellent summary of the rationale, development, and present status of multidimensional scaling. A number of difficult problems, many of which do not presently have adequate solutions, are discussed. These issues, which are somewhat similar to those

of factor analysis, are in general related to determining the appropriate Euclidean space for an empirically obtained matrix of scalar products. Of special concern is the lack of a test of significance for negative latent roots of this matrix and a completely satisfactory solution to the additive constant problem. The latter problem is investigated by Messick & Abelson (80).

In an attempt to evaluate a unidimensional scale, Milholland (82) applies Guttman's coefficient of reproducibility and proposes three additional indices of reproducibility. Auld, Eron & Laffal (7) have applied Guttman's scaling technique to the Thematic Apperception Test. As might be expected, difficulties in acquiring scalable items were encountered, although a reasonably satisfactory four-item scale of "sex drive" was obtained. A method of scalogram analysis in which a direct procedure based on summary statistics is used to compute a close approximation to the scale reproducibility, a formula for estimating the expected value of chance reproducibility, and an index of consistency for evaluating the reproducibility are given by Green (40). Guttman (43) proposes a new type of additive metric for perfect scales based on a non-Euclidean distance function.

Although the general problem of estimating parameters of normal bivariate and trivariate distributions containing incomplete data has been considered, no simple formulas for the maximum likelihood estimators are available. Special cases for the normal trivariate distribution are treated by Lord (69, 71) and Edgett (28). Lord discusses certain problems involved in equating test scores and derives maximum likelihood estimates for use in an equating procedure which specifies that equivalent population standard scores for each test are comparable. Levine (65) extended this work by developing formulas for equating tests based on parallel forms administered to nonrandom samples of different ability.

Of increasing interest to the psychologist is the problem of appraising the perceived social relations among individuals in groups whose members know each other well enough to make such judgments. A nonparametric approach called relational analysis has been presented by Tagiuri (106) in which diadic relationships existing in a group can be defined in terms of the member's choices, rejections, and perceptions of being chosen and rejected. Luce, Macy & Tagiuri (74) derived formulas for computing the expected frequency and variance of the different diadic forms expected, when certain random factors are taken into account. The technique shows great promise in an important field. Gardner & Thompson (33) have attacked the same general problem from a parametric point of view and have devised social-relations scales based on four of Murray's psychological needs and a variation of the fractionation procedure in psychophysical scaling. Evidence is given to support the claim for interval scales. Various indices of social group structure, their theoretical properties, and empirical relationships with morale indices are presented. A special feature of this procedure is the use of an "outside" reference population which permits comparison of the social status of an individual in different groups and a comparison of the social-relations status and structure of different groups.

FACTOR ANALYSIS

Although there have been relatively few articles on factor analysis during the past year, significant contributions have been made toward the understanding and solution of some of the continuing and crucial problems associated with this technique. Psychological questions, such as the choice of the mathematical restrictions to be imposed to determine the most appropriate mathematical model have been discussed. Old issues such as the relative merits of principal component and the Thurstone-type analyses plus new analyses attempting to synthesize as well as differentiate between the two have been presented. In addition certain statistical problems related to the determination of the number of factors to extract, an objective and unique rotation of axes to obtain simple structure, and tests of significance for factors have been considered.

In a most interesting paper Guttman (44) shows that a direct approach to factor analysis would involve finding factor score matrices η and ξ such that $\zeta = A\eta + \xi$ where ζ is the observed score matrix and A some real matrix of common-factor loadings. The usual procedures pivot on the observed correlation matrix, R . Real matrices A , L , and U are sought, where L is Gramian and U is diagonal, such that $R = ALA' + U^2$. The purpose of the paper is to examine the extent to which the indirect analysis of the scores ζ by means of the second equation is a necessary but not sufficient condition for the first. Six problems are explored in the process of setting additional conditions on this second equation. They involve the determinacy of η and ξ , communalities, meaning of factors, "inverted" factor analysis, second-order common factors, and rotation of axes.

Rao (97) stresses the importance of differentiating between the principal component analysis of Hotelling and the more common type of factor analysis discussed in psychological literature, both from the point of view of stochastic models and the problems of statistical inference. The need to specify the choice of restrictions to be imposed upon a vector space is emphasized and appropriate statistical techniques are reviewed for both familiar types of analyses. A new method called canonical factor analysis, including a test to determine the number of factors to extract, is developed. In canonical factor analysis the problem is to determine the factor variables which are maximally related to x . The solution to this problem depends upon a canonical correlation analysis of the hypothetical factor variables y with the measurable variables x of which y constitutes a part.

Rao points out that estimates of the factor loading obtained by his method can be shown to satisfy one out of a number of possible solutions to the maximum likelihood equations of Lawley (63, 64). One disadvantage of the procedure is the amount of labor required, since an iterative procedure must be used and convergence takes place slowly.

Another attempt to expand and generalize factor analysis theory has been made by Guttman (42, 46) in the development of his radex theory. Guttman proposes that the notion of order-factors, largely abandoned after Spearman's efforts, be reintroduced into factor analysis theory. He suggests that a new

type of hierarchy based on complexity of test be assumed. Tests, which differ only on a single complexity factor, are considered to be such that test t_{j+1} is more complex than t_j , and hence requires that which all preceding tests require, plus something additional. Let g denote the total complexity factor, of which all the tests are composed in various degrees. Thus g is like an additional test beyond the most complex test given. The basic hypothesis is that $r_{jg,k} = 0$, and as a consequence $r_{jk} = r_{jg}/r_{kg}$ where j and k are any two tests and $j < k$. Note that this criterion defines r_{jk} as the ratio rather than the product of the loadings as does the method of Spearman.

As a consequence of arranging tests meeting this criterion in a correlation matrix according to this type of hierarchy a definite gradient will be apparent. The largest value in any row (column) will be next to the main diagonal of that row (column), and the correlation coefficients will decrease as they depart from the main diagonal either to the left or the right. A set of tests whose observed intercorrelations satisfy this criterion condition is said to form a perfect simplex.

Guttman (46) generalizes the concept of a simplex by removing restrictions requiring only positive correlations and limited types of gradients and shows how this more general model is more appropriate to empirical data. By measuring distances between variables defined as points in a certain non-Euclidean fashion he presents the points as one-dimensional in this non-Euclidean system. The relationship of the generalized simplex to other factor analysis procedures and possible implications are discussed. This interesting approach seems to the reviewer to have in common with Scale Analysis a serious difficulty: empirical situations seldom meet, or even closely approximate, the conditions of the model.

During the past few years a number of methods for objectively determining simple structure have been proposed. Tucker (113), in an excellent paper which summarizes this work, evaluates a number of these methods in terms of 10 desirable requirements. In differentiating between exploratory studies and confirmatory factorial studies, he suggests that rotation in the first instance be considered an "art" and done subjectively while for the latter, objective determination of simple structure be required.

In terms of his 10 criteria he shows that available procedures represent approximations to simple structure with varying degrees of excellence. He proposes a new definition in terms of linear constellations. This method is shown to meet all 10 criteria, with the exception of not providing a statistical test to indicate the plausibility of accepting any particular solution as a simple structure. The approach is laborious and for practical use requires a high-speed automatic computer. Cattell & Cattell (17) discuss the rotation problem in terms of the "proportional profiles" procedure previously given by R. B. Cattell as a substitute for simple structure.

In situations where reasonable hypotheses about the simple structure of factor loadings of the variables are available, the laborious operations of rotation to simple structure may be reduced or circumvented by use of

Thurstone's multiple group method of factor analysis. Horst (51) presents detailed steps giving a simplified computational procedure for the multiple group method which he shows to be mathematically equivalent to the procedures of Thurstone (112) and Fruchter (32).

Interest in factor analyzing matrices of correlations other than those of test scores is still evident. Block (10) and Lorr, Jenkins & Medland (73) discuss the relationship between *Q* and *R* analyses and present situations where the two types of analysis yield different outcomes. Nunnally (91) presents an argument for obtaining item weights based on factor loadings obtained from factor analyzing over "persons."

Notes presenting two simplified computational procedures are given by Cureton (24) and Kaiser (58).

COMPUTATIONAL AIDS

A number of useful tables have already been mentioned in previous sections of this review. Additional computational aids of interest to the psychologist follow. Jenkins (55) presents tables which permit the computation of tetrachoric correlation coefficients without interpolation and with a mean error of less than .005. This is achieved by application of a tabulated correction factor to take into account the positive bias in tetrachoric correlation coefficients computed directly from cross-products where the splits vary greatly from the medians. A second device, which has value in correlational problems, is a nomograph devised by Lord (72) for obtaining multiple correlation coefficients from the intercorrelations between two predictors and a criterion measure.

Although a number of methods for simplifying estimation of the statistical significance of differences between proportions or percentages are available, Petersen (94) offers two new useful nomographs, one for independent samples and the other for correlated samples. They are easy to use but are appropriate only with samples of the same size. Tables to give quick estimates of the significance of differences between means are presented by Jackson & Ross (54). They are based upon an analogue of the *t*-test in which range is used instead of the standard deviation. The advantages and efficiency of using techniques based on range are discussed by Noether (90). Tukey (114) presents methods for interpolations in tables of the normal range such as David's, and Hartley and Pearson's. He reports "surprisingly good approximations." The distribution of the quadratic function $Q_k = \sum_{i=1}^k a_i x_i^2$, $k=2,3$, where the x_i 's are normally and independently distributed with zero mean and unit variance, $\sum a_i = 1$, and $a_i > 0$ is tabled by Grad & Solomon (38).

In view of the increasing use of the Kolmogorov-Smirnov statistics, a welcome aid is a more extensive table of percentage points for this test prepared by Miller (83). Tables for $N=1-100$ and $\alpha=.10, .05, .025, .01$, and .005 are given.

In an editorial (93) in *Biometrika*, tables of certain area-ordinate ratios and their reciprocals from the normal probability function are given. These tables have special use in such procedures as normalizing data and computing biserial correlation coefficients. A brief description of a number of unpublished tables associated with normal, gamma, and t distributions is presented by Teichroew (110).

A general treatment of the tabular method suggested by Yates for factorial design experiments is presented by Bainbridge, Grant & Radok (8), using any number of factors at any number of levels, not necessarily the same for each factor. Procedures utilizing punch cards are described and illustrated.

An interesting and effective use of mark-sense cards in test research is reported by Appel & Cooper (3). The procedure has the advantage of not being limited by the 27 column capacity of the standard mark-sense card and incorporates a machine checking procedure which permits easier editing of the multiple-coded columns. An application of the method in a study involving a 432 item True-False questionnaire administered to 500 college students is described.

MISCELLANEOUS

Occasionally psychologists, especially social psychologists, are concerned with sampling from large finite populations. In view of the increasing importance of research with such populations a few articles dealing with sampling procedures and resulting statistical tests are noted.

Moser (85) describes the main developments in the sampling of human populations in Great Britain during the past five years. Changes in methodology, as well as new applications in various areas of research such as educational, social, economical, and public opinion, are included. Jones (56) studies the relative merits under varying conditions of two sampling plans in terms of the adequacy of the estimates of the population mean and its variance error. Both plans involve dividing the population into a given number of slices. In the first plan, originally proposed by Tukey, multiples of a constant k are added to the serial numbers of the initial randomly selected sample items, one being taken from each slice. The second, used so frequently by Deming, selects successive items from each slice by random sampling without replacement.

Deming (25) suggests a method based on equal work loads for interviewers which includes replication of the sampling procedure with equal probabilities. By setting up arbitrary zones containing the same number of work loads, it is possible to take advantage of replication and equal probabilities, and utilize single stage sampling analyses.

Lieberman & Solomon (67) present a multi-level sampling plan which specifically allows for any number of sampling levels, provided that transitions can only occur between adjacent levels. This plan is a random walk model with reflecting barriers. Moses (87) considers some theoretical aspects

of the Lot Plot Plan which is a modified variables sampling plan using 50 observations in groups of five and making use of the sample histogram. This procedure has been widely used in industry to detect nonnormality of distribution.

A few of the numerous papers which discuss the properties of various statistics and sampling distributions are herein considered. In an excellent, very readable paper, Chernoff (18) presents a review of some of the literature concerned with large sample theory. Hitherto neglected theorems and fields are considered.

Fiske & Baer (30) critically examine the coefficient of variation. Its sampling error, use, and previous application in a number of studies are reviewed. The authors believe that it has limited utility and should be restricted to occasional use as a descriptive statistic. Anis (2) studies the variance of the maximum of the partial sums of N independent standard normal deviates.

The hypothesis that two samples come from the same rectangular population can be tested by means of the quotient of the maximum values. Murty (88) derives the distribution of this statistic and studies its properties. A numerical example and a table of the 5 per cent points of this statistic for sample sizes ≤ 10 are given. Rider (98) supplements the work of Murty by deriving the distribution of the product of maximum values in random samples from a rectangular distribution.

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COMPARATIVE PSYCHOLOGY¹

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Over half of my allotted 15 pages would be required simply to list the more than 200 recent studies that are relevant to the work of the comparative psychologist. Therefore, in order to maintain a reasonable balance between citing and reviewing, I have had to exclude from this review many important papers published in the past year. My selection procedure was rather arbitrary. First I excluded all studies dealing with sensory, perceptual, and learning capacities, and with social behavior. These areas of investigation have been reviewed extensively in recent volumes of this publication. Then I classified the remaining papers into a number of problem areas and selected four of these areas for discussion. Finally, within each area, I selected for review the more recent studies; however, I did not hesitate to cite older publications where they provided some historical continuity or were otherwise relevant. The four areas covered in this review are as follows: the relation of ethology to comparative psychology, instinctive behavior, factors affecting behavioral development, and the study of specific behavior patterns.

ETHOLOGY AND COMPARATIVE PSYCHOLOGY

It has become customary to make a distinction between rat psychology and comparative psychology, and to point out that the American animal studies are concerned more with the rat than they are with species comparisons. Perhaps, in a way, this is true. But it seems time now to recognize also that general investigations of the behavior of a wide variety of phylogenetically differentiated species, the type of investigations in which ethologists seem particularly interested, likewise do not necessarily qualify as comparative studies. Nissen & Semmes (58) have pointed out that, before any meaningful phyletic comparisons of behavior can be made, it is necessary to delineate the behavioral dimensions and analyze the neural mechanisms which describe the significant aspects of animal behavior. This I gather also to be the gist of Russell's (69) view that crucial comparisons must involve behavioral phenomena that are truly homologous rather than merely analogous. It is as easy to lose sight of this important consideration while studying the behavior of a large number of species as while working with only one species. If American comparative psychologists have a fixation on the rat, the European ethologists are spreading their libido too thinly on too many species. In the extreme, both approaches can be unhealthy.

Characteristically, the psychological studies of animal behavior are experimental-analytic, while the ethological ones are naturalistic or observa-

¹ The survey of the literature pertaining to this review was completed in April, 1956.

tional. (It is about as rare to find an analytic investigation in the two ethology journals, *Zeitschrift für Tierpsychologie* and *Behaviour*, as it is to see a naturalistic study in the *Journal of Comparative and Physiological Psychology*.) Scott (73) has recently re-emphasized that both these approaches are essential to sound scientific (particularly biological) research, and this is true enough. What should be noted in addition is that the significant dimensions of behavior required for phyletic comparisons can be reliably delineated only by examining the variations in behavior under controlled experimental conditions. Such experimental analysis requires bringing together the results of a large number of investigations on the same species; isolated studies of many different species do not contribute to analysis. Thus, the most promising course for comparative psychologists appears to be that of concentrating on highly analytic studies of a few well-selected species (perhaps the rat, cat, dog, monkey, chimpanzee, man, and some convenient laboratory species of birds, reptiles, fishes, insects, and worms). Since, except for a few notable exceptions [e.g., Lorenz (45, 46); Tinbergen (82)], ethological studies are more naturalistic and ecological than analytic in their orientation, they are not as directly relevant to phyletic comparisons of behavior as they might appear to be.

The value of ethological studies² lies rather in their systematic observations and description in general terms of the behavior of a wide variety of species in their natural surroundings. The ingenious little experiments reported in these studies often help to make the descriptions more complete and exact, and, as Hess (32) has pointed out, the ethologists have undoubtedly accumulated a substantial body of reliable observations. The recent issues of the two ethology journals, as well as of the *British Journal of Animal Behaviour*, contain a virtual mine of valuable and interesting information on the life and habits of, for example, the fly (4), ant (84), bumblebee (21), tropical fish (3), curlew (23), domestic chicken (87), shrew (11), lynx (44), sea lion (18), elephant (64), monkey (59), and many other vertebrate and invertebrate animals. The descriptions of feeding habits, sexual behavior, care of the young, and the like in these species are valuable in that they provide the type of information that must be available to the comparative psychologist if he intends to undertake analytic studies of specific behavior patterns. In general, it is this kind of background information, necessary for further experimental work, which is supplied by the painstaking work of ethologists. However, it is important to realize that their naturalistic studies, though often an essential first-step, are quite removed from the type of analytic studies that form the immediate basis for direct phyletic comparisons.

² I am ignoring the nonethological and nonanalytic papers that also sometimes appear in the two ethology journals. Most of these papers are nothing more than anecdotal reports on such diverse items as observations of "unusual" sexual behavior in donkeys and horses (2), telescopic observation of four copulations in the wild rabbit in Tasmania (67), procedure used by mahouts for taming the wild Indian elephant (63), and the adaptation of one young swallow of unknown history to the conditions of captivity (38)!

INSTINCTIVE BEHAVIOR: A NEW FRAMEWORK

Theory.—The term "instinctive behavior," as used by modern psychologists, is simply a descriptive label for activities which involve complex sequences of acts and yet appear to be unlearned: activities such as hoarding in the hamster, maternal behavior of the rat, homing and nest building in birds, and dancing of the honeybee. The adjective "instinctive" serves merely to identify this rough class of behavior, leaving open the question of the exact role of learning and other mechanisms in determining the specific behavior patterns. Though popular at one time, the notion that any given instance of instinctive behavior can be explained in terms of a unitary, innate process, called "instinct," was widely discarded by psychologists some time ago. However, Lorenz (46) and Tinbergen (82) have recently reintroduced, without much reference to psychological literature, the concept of instinct as an explanation of instinctive behavior and have sought to link it to hypothetical physiological mechanisms. The essence of this view is that the activation of an hypothetical, innate releasing mechanism by an appropriate stimulus releases the specific energy corresponding to the innate, stereotyped, instinctive act, which, once released, moves toward completion without further dependence upon receptor activity.

This new instinct theory has already been examined in detail and criticized by Lehman (43). He found the writings of Lorenz and Tinbergen on this subject to suffer from "inappropriate and gratuitous" categorization, preconceived and invalid notions of innateness and maturation, analogical reasoning, and teleological conceptions. To this vigorous, perhaps too vigorous, attack from the viewpoint of American psychology have now been added three evaluations by European authors. First, Kennedy (36), a British worker, points out that the Lorenz-Tinbergen concepts of action-specific energies and displacement reactions are based on confusion between behavioral and physiological descriptions and neglect of the role of external stimuli in determining response sequences. Second, Kortlandt (39), a zoologist at the University of Amsterdam, has examined the concept of hierarchy as used in the Lorenz-Tinbergen instinct theory. Though Kortlandt himself subscribes to the idea of the hierarchical organization of instinctive goals, he considers as inadequate both the behavioral and the physiological aspects of the specific interpretation of hierarchy given by Lorenz and Tinbergen. Third, the genetic aspect of Lorenz's instinct theory, as developed in his discussion of domestication, has been criticized by the geneticist Spurway (75). In a closely reasoned paper on the causes of domestication, she questions the validity of Lorenz's evolutionary theory and experimental practice and attributes his error to lack of familiarity with the rationale of inductive and deductive population studies as conducted by geneticists.

In spite of the interest evidenced in these general discussions, experimental tests of specific aspects of the Lorenz-Tinbergen theory have not appeared in large numbers. I have found only two investigations that bear directly on this theory. Hirsch, Lindley & Tolman (33) experimentally tested the Tinbergen hypothesis that certain specifically shaped stimuli are in-

nately related to fear response in fowl. Their data, obtained from the White Leghorn chicken, failed to support the hypothesis. An almost identical investigation was carried out independently by Rockett (65) in the White Rock chicken. His results were similar to those of Hirsch, Lindley & Tolman. He attributes the Tinbergen results to the operation of some uncontrolled variable.

Empirical studies.—Investigations of instinctive behavior can be grouped into two main categories: descriptive studies, which identify and describe patterns of behavior that may reasonably be classed as instinctive; and analytic studies, which seek to determine the mechanisms underlying the various instances of instinctive behavior. Examples of descriptive studies reported during the last year are Schmidt's study (71) of the nest building behavior of the termite, an investigation of the songs of the hedge sparrow by Sauer (70), and observations by Keenleyside (35) on the schooling behavior of fish. In general, papers of this kind tend to consider the observed behavior patterns as "innate," or attribute them to instincts, without any serious attempt to investigate the underlying mechanisms. Perhaps there is some truth in the remark that the degree of assurance with which certain behavior patterns are attributed to the operation of an instinct is inversely related to the extent to which they have been experimentally analyzed.

Analytic studies of instinctive behavior during the past year have been concerned mostly with hoarding in rodents and navigation in birds. *Bird Navigation* is the title of a recent book on this subject by Matthews (49). The author reviews the literature on the problem and then offers an explanation of his own. He interprets the ability of certain birds to fly over unfamiliar terrain to their homes in terms that would make the birds capable of computations and extrapolations as elaborate as those involved in guided missiles. In essence, the hypothesis states that the bird selects the direction of flight on the basis of certain inferences made from observations of the position and movement of the sun. Matthews' argument is not convincing, and there are at least two recent experiments, by Kramer (40) and Rawson & Rawson (61), which present evidence against the sun-navigation hypothesis. There appears to be no doubt that most birds are capable of navigating to their homes from great distances, but no acceptable explanation of the phenomenon has been offered so far. A new line of research is evident in the work of Kramer, Pratt & von St. Paul (41), recently reported in *Science*. They found that pigeons displaced to the south were more successful in reaching home than were the pigeons displaced to the east, west, or north (least successful). Investigators interested in bird navigation will undoubtedly be eager to read a detailed report and interpretation of these experiments.

Interest in the study of factors related to hoarding shows no sign of waning. Gross, Fisher & Cohn (25) have shown that rats, after they have been fed a rachitogenic diet for about 40 days, hoard normal pellets in preference to rachitogenic pellets. Smith & Powell (74), using three inbred strains of mice, found hoarding to be related to certain indices of emotionality. The

observed relations were quite complex, and there is likely to be some disagreement about their interpretation. An investigation by Stamm (76), designed to test the hypothesis that hoarding is related to aggressive behavior, produced negative results. Ross, Smith & Woessner (66) conclude their review of the literature on hoarding by the statement that the studies "do not seem at this time to fit together into any meaningful scheme." I feel this complaint is justified. Recent work does not appear to be aimed directly at (a) finding the essential variables relevant to hoarding and (b) formulating a theoretical interpretation. Reading through the studies one gets the impression that the investigators are either employing a shot-gun approach to the understanding of hoarding behavior or are being discouraged from putting their theoretical rationales and interpretations into print.

A new framework.—In a recent article, "The Descent of Instinct," Beach (5) presents a timely reappraisal of the problems presented by instinctive behavior and offers a new framework for empirical studies in this area. Beach first traces briefly the history of the concept of instinct from the Greeks, through Thomas Aquinas and Rene Descartes, to the Darwins and McDougall. After pointing out the reasons for the failure of the anti-instinct revolt of the twenties, Beach criticizes the current approach to the problem of instinctive behavior. He cites evidence to show that genes are just as important in determining learned patterns of behavior as in determining instinctive patterns; therefore, genetical determination cannot serve as a criterion for differentiating between instinctive and noninstinctive behavior. Further, the genetic and learning factors do not exhaust the universe of factors determining behavior; thus even if a behavior pattern is shown to be unlearned, it is illogical to conclude that the pattern is primarily genetically determined. "The final form of any response is affected by a multiplicity of variables, only two of which are genetical and experiential factors" (5, p. 405). Beach seems to recommend two types of analyses of instinctive behavior and indeed of behavior in general: determination of the relation between genes and behavior, and study of the experiential as well as nonexperiential historical factors (including the prenatal environment) in the development of behavior.

Hebb (29, 30) has already enumerated and discussed the various classes of variables relevant to the development of behavior: (a) genetic, (b) chemical characteristics of the uterine environment of the embryo, (c) postnatal chemical environment of the nervous system, (d) learning through sense-organ stimulation that is common to all members of the species, and (e) learning through sense-organ stimulation that is variable from individual to individual. Investigations involving the controlled variations of these development factors (especially a, b, and d) seem to this reviewer to be the next phase in the experimental analysis of instinctive behavior. Indeed, the phase has already started (see the following section) and, if its initial spurt is any indication of its progress during the next decade, it is likely to provide an unceremonious but respectable burial for the concept of instinct as an explanatory construct in psychology. The facts and theories that

now appear under the rubric of "instinctive behavior" would then find their place in discussions of the role of these developmental factors in determining behavior.

FACTORS AFFECTING BEHAVIORAL DEVELOPMENT

"Constitutional factors."—The effects on behavior of variations in development are in general hard to attribute to any specific developmental factor, but it is particularly difficult to separate the effects of the genetic and prenatal-environmental factors. A fertilized egg in a cat's womb, for example, has not only the "cat genes" but it also develops in a cat's uterus, and the kitten is a product of both the genetic factors and the intrauterine environment. Since these two factors are seldom varied independently of each other, we can use the term "hereditary," or "constitutional," factor to refer to their combined effect.

Mahut (48) has recently completed an investigation of the role of constitutional factors in the emotional behavior of dogs. She observed the reactions to strange objects of 202 pure-bred dogs belonging to 10 different breeds. She found the terriers, boxers, and bulls to be less wary and more aggressive than other breeds such as the dachshund and collie. She attributes these differences to constitutional factors but also stresses the importance of complex (postnatal) environmental stimulation for making these behavioral differences evident. Fuller (22) has also studied breed differences in dogs. He was interested in differences in "trainability." Unfortunately, because of the operation of certain confounding factors, he was unable to attribute the observed differences solely to constitutional factors. Valenstein, Riss & Young (83) have shown that the age at which the copulatory pattern develops in the male guinea pig is different for constitutionally different strains of animals.

Intrauterine environment.—Difficult though it is to isolate the effects of variation in genetic factors from those of the variation in intrauterine environment, it is not impossible. The intrauterine environment can be varied independently of the genetic factors in two ways. One of these involves implanting fertilized eggs from the female of an in-bred strain into the uteri of females of different strains or characteristics. In this kind of transplantation the genetic characteristics of the fertilized eggs are unaltered but their intrauterine environment varies. Ginsburg & Hovda [(24), cited by Beach & Jaynes (6)] have successfully used this technique in their study of audiogenic-seizure susceptibility in mice. The other method of obtaining variations in the prenatal environment involves experimentally changing the intrauterine environment itself. A recent experiment by Thompson & Hockman (79) illustrates this method. They subjected pregnant rats to a conditioned stimulus that had previously (before pregnancy) been associated with electric shock. (No shock was administered during the pregnancy.) The rats showed the usual conditioned anxiety. After parturition the offspring of these stress-females were compared with those of females in the control group, which had been shocked and exposed to the stimulus randomly before preg-

nancy. The offspring of the females exposed to stress were found to be more "emotional" (open-field test) than those of the control females, even when the postnatal nursing experiences were equivalent for the offspring of both groups. Awaiting the results of a replication study, Thompson & Hockman think that the differences in emotionality may be related to possible changes in the fetal-maternal blood stream brought about by "anxiety" in the females of the stress group.

Early experience.—Investigators interested in the effects of early experience on behavior are in the enviable position of having three reviews of the field available to them. To the 1954 review by Beach & Jaynes (6) have now been added two others, one by Thompson (78) and the other by Drever (16). These reviews are complementary rather than overlapping. Beach & Jaynes grouped the studies in terms of their relevance to specific functions: learning, feeding behavior, reproduction, emotion, and so on. Thompson pays special attention to the influence on behavior of prenatal factors; he also compares the effects of early and late postnatal restricting experience. Drever is more theoretically oriented, seeking to bring together the clinical, ethological, and experimental material within a single, general framework.

Studies of the effects of early experience involve the exact statement of three variables: the characteristic or dimension of behavior, such as emotionality or problem-solving ability, which is to serve as the dependent variable; the differences in the experience given to the controlled and the experimental animals; and the age at which the differential treatment is given. One of the latter two serves as the independent variable. I have chosen to arrange the year's empirical studies with respect to the type of independent variable employed.

There appears to be considerable interest in the possible developmental effects of "gentling" in infancy; however, the results of the investigations to date are equivocal. Gentling consists of holding the animal lightly and stroking its back from neck to tail. In a series of experiments Weininger (85) found that, compared to the nongentled animals, the rats that were gentled for 10 min. a day over a 21-day period immediately following weaning weighed more, defecated less, showed greater ambulatory activity in an open-field type of situation, and were more resistant to organic damage under severe stress. The stress consisted of immobilizing the animal and depriving it of food and water. Contradicting these results is the study of Scott (72). He compared the effects of gentling, ignoring, and shocking rats during infancy. Scott reports no differences between the groups in per cent of original weight gained, activity in a strange situation, or resistance to stress. The strain of rats, as well as some of the details of apparatus and procedure, was not the same in the two studies, and this may account for the contradictory results. While there has been some speculation about the effective element and the physiological mechanisms involved in the possible gentling effects, there seems to be little point in discussing them until we know exactly what aspects of behavior, if any, are affected by gentling during infancy.

McClelland (50) compared weight gain in five groups of rats which varied

in the tactile stimulation (gentled, brush stroked, held in hand, restricted in box, and isolated) they received between the ages of 21 and 42 days. The gentled and brush-stroked groups were found to gain more weight than the three other groups. McClelland states that the difference in weight gain is attributable to differences in tactile stimulation and not to warmth caused by holding the animals, but he presents no direct controls for body warmth (or for activity). The mechanism underlying a related finding has already been discovered. King & Connon (37) found that, after weaning at 15 days of age, young mice raised with other mice were more viable than those raised in isolation. By experimentally varying the temperature of the environment, they were able to attribute the difference to heat conservation in group living.

Another type of independent variable employed in the studies of the effects on behavior of early experience is restriction. Restriction may be perceptual, motor, or both. In another part of her study mentioned above (48), Mahut, using Scottish terriers and boxers, compared the emotional behavior of three groups of dogs of the two breeds. One group was home-reared, one kennel-reared (semi-restricted), and the third was reared in laboratory cages under conditions of severe perceptual and motor restriction. She observed marked difference in emotional behavior between the three groups; the kennel group showed more avoidance responses, and fewer teasing and approach-avoidance responses, as compared to the home-reared group. The predominant response of cage-restricted dogs was diffuse excitement, a result that agrees with the previous findings of Melzack (52). Luchins & Forgas (47) compared an experimental group of rats, reared for 52 days in a varied, "stimulating" environment, with a comparable group reared in ordinary laboratory cages. They found the experimental animals to be more active, less "emotional," more variable in their choice of paths to food, and better able to learn detour solutions to a maze problem.

There are now many demonstrations that early restriction produces significant effects on adult behavior. The next logical step is to study the effects on different aspects of behavior of different types of restriction. Work on this problem has been started by Forgas (19, 20). He reared a group of rats with ample visual and motor experience, and an equivalent group with visual experience but only limited opportunity for motor experience. Then he compared the two groups on maze learning with visual cues and with reduced visual cues. The first group learned better than the second under the conditions of reduced visual cues, but not under normal conditions.

These studies raise an important theoretical question. How general are the effects of early perceptual and motor experience? Hebb (28) considers perceptual experience to be important not only for specifically perceptual abilities (e.g., discrimination) but also for problem solving, general intelligence, and emotional behavior. Forgas seems inclined to favor a limited-effects interpretation. He concludes that

early visual experience improves discrimination and problem-solving ability in situations where visual cues are the primary aids to solution of the problem, and that

complex motor experience improves learning and problem-solving ability in situations where kinesthetic cues seem to be important for solution of the problem (20, p. 213).

It is to be hoped that future research in this field will be directed towards clarifying this theoretical issue.

The variable of age seems to this reviewer to be the most important parameter in the studies of the effects of early experience. The question is as follows: what are the differences in the effects of equivalent experiences given at different ages? The critical-age aspect of the phenomenon of "imprinting" clearly refers to age as the crucial variable. Considerable interest has been aroused by this phenomenon. Contrary to earlier descriptions, Steven (77) has found imprinting not to be irreversible. Thorpe (81) has pointed out that the factor of secondary reinforcement has not been adequately controlled in most studies of imprinting. Hess and his collaborators (see 32) are now engaged in experimental analysis of the phenomenon.

Apart from imprinting, investigations involving age as the independent variable are also relevant to the general problem of the way in which perceptual and motor abilities develop in the infant. A study by Drever (15) bears directly on this problem. Drever compared the performance on three spatial tests of three groups of human adolescents: early blind, late blind, and normal. He interprets his results as pointing to the existence of certain basic (perceptual) skills which are built up slowly early in life and on which later experience has little effect. Unfortunately, as far as I could see, there are no other recent empirical studies dealing with the variable of age.

STUDIES OF SPECIFIC BEHAVIOR PATTERNS

Exploration.—There has been considerable recent interest in the exploratory activity of animals. Exploration is measured (and defined) in such terms as frequency of approaching and sniffing an object, head-orientation movements, the extent and pattern of perambulation through a standard observation area, visual exploration or frequency and duration of looking at environmental stimuli, manipulatory exploration or time spent in manipulating stimulus objects, and so on. Investigators have studied the relation of exploratory activity to a variety of variables: Berlyne (7) found exploratory activity of well-fed and well-watered rats to increase in an environment presenting numerous and complex stimuli, and to decrease after an animal's first experience in a situation. It remained unaffected by a slight change in a familiar environment, by the albedo of objects, and by confinement of the animal just prior to the exploration test. Butler & Alexander (9) observed that rhesus monkeys repeatedly spent about 40 per cent of a 10 hr. testing period in visual-exploratory activity. Thompson & Kahn (80) demonstrated that a maze task interpolated between two exploration tests can have retroactive inhibitory and facilitatory effects on exploratory activity of rats. Welker (86) observed that chimpanzees explore more when they are presented with novel rather than familiar stimulus objects and that the younger animals explore more than the older. Chance & Mead (10) found

that the addition of a new object to a familiar environment increases exploratory activity more than does the removal of the object. Darchen (12) was able to identify exploratory activity in the cockroach and to study the effects on it of vibrations and illumination.

Other investigators have been concerned with the relation between exploratory activity and drive states. Adlerstein & Fehrer (1) found that hungry rats explore more than sated rats, but both hungry and sated animals show a similar decrement in exploration with increasing familiarity with the situation. Montgomery & Monkman (55) demonstrated that "fear" induced immediately prior to the exploration test does not affect exploratory activity in rats, but fear induced during the test reduces it. In another study, Montgomery (54) observed less exploratory activity in the dangerous, elevated alleys than in the safer, enclosed alleys.

Theoretically, exploratory activity has been viewed in two different ways. Berlyne (7), and Montgomery and his collaborators (54, 55, 56), postulate a curiosity or exploratory drive corresponding to other biological drives. This drive differs from the homeostatic drives, such as hunger, in that it is aroused by (novel) external stimulation rather than by the internal state of the organism, and it is satiated quickly by continuous exposure to the same stimulus situation. This idea of curiosity or exploration as a drive is supported (a) by Dember (13), who argues that certain results from T-maze alternation experiments in rats cannot be explained in terms of stimulus satiation but require the postulation of a positive exploratory drive, and (b) by Montgomery & Segall's finding (56) that opportunity to explore can serve as a reinforcement for simple black-white discrimination learning in the rat. Harlow (26) has also interpreted the experimental results from the Wisconsin laboratory, showing that visual exploration and manipulation have reinforcing properties, in terms of "exteroceptive" (rather than homeostatic) drives. [Dennis (14) points out that around 1900 both Romanes and Thorndike had observed and reported "intrinsically motivated" problem-solving behavior in monkeys; however, external rewards became so popular with experimental psychologists that exploration- or exteroceptively-motivated learning and performance had to be rediscovered.]

The second interpretation of exploratory activity is the one presented by Hebb & Mahut (31) and is based on some experiments conducted at the McGill laboratory. They observed that when a hungry rat is offered two routes to food, one direct and the other longer and with blind alleys, the animal has a marked tendency to take the more difficult route to the food. An experiment by Havelka (27) has shown this even more clearly. He gave hungry rats a choice of obtaining food from either of two equally familiar situations. However, in one situation the food was always to be found in the same location, whereas, in the other, the position of the food was not only at a greater distance from the starting point, but was varied from trial to trial so that the animal had to search for it. Havelka found that some animals consistently preferred the complicated, variable-goal situation to the simple, fixed-goal situation. Both Hebb & Mahut and Havelka argue

that the behavior of these animals is something more than exploratory activity. As postulated, exploratory drive is aroused by novel stimuli and is quickly satiated. However, their animals were quite familiar with the experimental situation and yet they chose the more difficult solutions to the problem presented by the maze; furthermore, their animals switched to the easy, direct solution as soon as they changed the variable-goal situation to a fixed-goal one. These authors consider the behavior of their rats to be problem seeking rather than merely exploratory. Following Berlyne (7), Montgomery & Segall (56), and Harlow (26), are we then to postulate a problem-seeking drive in addition to the exploratory drive? Hebb & Mahut (31) feel, like Nissen (57), that the time has come to interpret the variety of phenomena reported above in terms of a general unifying concept. Hebb & Mahut say that the various types of experiments reported in this section are special cases of the general tendency of mammals "to seek perceptual change and novelty"; research efforts should, therefore, be directed toward finding what constitutes novelty or perceptual change, and how this varies with phylogenetic level.

Hunger and eating.—The New York Academy of Sciences has published a symposium on the regulation of hunger and appetite. The comparative psychologist will find little of interest here, except a paper by Miller (53). In this paper, Miller briefly describes and compares some of the behavioral measures of hunger drive. A technique for quantifying hunger behavior has been developed by Duckworth & Shirlaw (17). They have designed an apparatus for recording the jaw movements of cattle; the record provides such measures as total number of bites, time spent eating and cuddling, and miscellaneous jaw movements. It is to be hoped that this technique, and its adaptations, will facilitate quantitative, analytic studies of the grazing behavior of sheep and cattle, a subject on which many naturalistic papers have previously appeared in the *British Journal of Animal Behaviour*.

The important problem of feeding rhythms and their relation to food intake and hunger drive has been studied by three different sets of investigators during the past year. Reid & Finger (62) switched mature rats from an *ad libitum* feeding schedule to a 23 hr. food-deprivation cycle and measured changes in body weight, food and water intake, and activity. They found that progressive changes in all measures continued for at least 15 days, and prefeeding activity was still increasing at the end of 35 days. Lawrence & Mason (42) compared the food intake of a "periodic group," which was fed for 3 hr. each day at the same time, with that of an "aperiodic group," which was also fed 3 hr. at a time but after deprivation varying from 4 to 48 hr. After 27 days of this differential treatment, the food intake of the rats in both groups was found to be proportional to the interval of deprivation (up to 24 hr.). Also, for a given deprivation interval, the periodic group was found to eat more if tested at the regularly scheduled time of day than if tested at any other time. Ramond, Carlton & McAllister (60) found that the limited-time feeding versus limited-amount feeding variable is related to body weight and food-reinforced running speed in rats. Apart from

their contribution to the analysis of "hunger behavior," these three papers clearly point out that the feeding rhythms existing before an experiment may affect results of experiments involving food deprivation. Therefore, in reporting results it is important to specify, not only the hours of deprivation, but also the details of the change in the deprivation-feeding schedule under experimental conditions. This general point concerning the adequate control of developmental factors in experiments on specific behavior patterns has been discussed by Bindra (8).

Emotional behavior.—One of the basic problems connected with emotional behavior, a problem that continues to be neglected, is that of determining the exact nature of the stimuli that evoke emotional excitement. In 1949 Hebb (28) proposed, in essence, that novel, or unusual combinations of familiar, perceptions produce emotional excitement. Since that time Melzack (51) and Mahut (48) have successfully employed stimulus objects such as mechanical snakes, inflating-balloons, and putting-on-face-masks to evoke emotional excitement in dogs. Other investigators have made use of other objects that also fit in the general class of "unusual perceptions." However, the exact effective elements in these stimulus-objects, the elements that constitute perceptual novelty for various ages and species of animals, remain yet to be determined. Another related problem is to determine the extent to which Hebb is correct in hypothesizing perceptual novelty as the sole, or at least the most important, external condition for evoking emotional excitement. These are basic problems and they deserve, it seems to me, more attention than they have hitherto received.

The way in which specific, organized patterns of behavior, such as withdrawal and attack, develop from unorganized, general excitement is another significant research problem. In general terms we know that with repeated exposure to essentially the same "emotional situation," general excitement is gradually replaced by organized (goal-directed or stereotyped) response patterns. However, as I have pointed out elsewhere (8), we do not know what specific factors determine the organized response patterns (withdrawal, attack, "friendliness") that finally emerge in a given situation. It seems important to investigate the "innate" and experiential factors that determine the development of various organized response patterns.

Still another important problem is that of finding and defining dimensions along which the various so-called emotional patterns of behavior can be meaningfully described. A beginning in this direction is to be found in a factorial study of "emotionality" by Royce (68). He administered 32 physiological, psychological, and social-behavior "tests" to 53 pedigreed dogs belonging to six breeds. Six of the 10 factors extracted from the correlation matrix could be identified as Timidity I (freezing and energy arousal), Timidity II (withdrawal and hypo-activity), Heart Reactivity (heart-rate changes in social situations), Aggressiveness (agitation in response to social and inhibitory stimulation), Activity Level, and Audiogenic Reactivity (hyperreactivity to auditory stimulation). [In connection with the two timidity factors it is interesting to note that Kamin *et al.* (34), in interpret-

ing the results of a study of anxiety in human subjects, also postulated two separate variables. These two variables, avoidance tendency and general upset or arousal, seem to be similar to Royce's withdrawal and energy arousal.] The factorial approach to the problem of delineating dimensions of emotional behavior is probably quite useful at this exploratory stage; however, once the dimensions of emotional behavior have been defined, we shall have to turn to experimental studies to determine what conditions affect the defined aspects of emotional behavior. This will pave the way for species-comparisons of emotional behavior.

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PHYSIOLOGICAL PSYCHOLOGY^{1,2}

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The search for the physiological mechanisms underlying behavior has been greatly spurred in recent years by the tremendous advances neuro-physiologists have made in our understanding of the organization of the nervous system. Not only has the physiological psychologist been afforded new techniques and new concepts of neural function, but it has become abundantly clear to the neurophysiologists themselves that behavioral techniques and behavioral concepts are essential in the analysis of the complexities of the nervous system. Perhaps nowhere are these developments more evident than in the area of cognitive functions where the highest level of organization of the nervous system is reflected and where our need for new approaches is the greatest. A survey of the literature of the past year makes it clear that we actually are on the threshold of a significant breakthrough in our understanding of the neural basis of complex functions like consciousness, attention, learning, memory, and intelligence. This review, therefore, will concentrate on the year's progress in these important cognitive functions and, because of the limitations of space, will only briefly summarize the highlights of advances made in other facets of physiological psychology.

Much has stood in the way of successful attack on complex cognitive functions in the past. In the first place, objective psychologists have traditionally shunned the question of consciousness, even denying its existence, and have not been able to develop concepts of attention, intelligence, and even learning and memory that are satisfactory for physiological analysis. Second, it has been a costly error of the past that we have all too often thought of cognitive functions as simple, unitary processes and have neglected the fact that the operational definitions derived from our laboratory tests are limited and, at the same time, contaminated by the contribution of many variables like emotion, motivation, sensory capacity, attention, etc. Third, up until recent years too much of our information was biased by the almost exclusive use of simple preoperative-postoperative tests of the effects of clean, surgical lesions. A fourth, and most important, obstacle has been the almost universal faith that the neural basis of cognitive functions will be localized in specific pathways and in restricted anatomical loci, particularly in the cerebral cortex.

Many of these difficulties of the past are now widely recognized, and

¹ The survey of the literature pertaining to this review was completed in April, 1956.

² The following abbreviations are used in this chapter: CR (conditioned response); CS (conditioned stimulus); ECS (electroconvulsive shock); EEG (electroencephalogram); UCR (unconditioned response); UCS (unconditioned stimulus).

approaches to the question of the physiological basis of cognitive functions are being improved and diversified with heartening success. The neurophysiologist, unhampered by the psychologist's sophistication and theoretical struggles, has boldly led the attack on some of the most complex, and hitherto forbidding, problems in psychology (12). Surgical ablation has been supplemented by extensive use of electrical stimulation and recording in chronic waking preparations, by intracranial injection of chemical solutions, and by experimental production and removal of discharging lesions in the brain. Furthermore, the obvious theoretical importance of the rhinencephalon and the brainstem and thalamic activating mechanisms and the spread of stereotaxic methods have directed more and more experimental attention away from the neocortex and toward subcortical mechanisms where both electrolytic lesions and implanted electrodes have been most fruitful. And, perhaps most important of all, we are coming to abandon the extremes of strict localization and "field" theories and are beginning to think, as Lashley (38) has suggested for many years, in terms of widely dispersed and dynamic, physiological properties of complex anatomical systems. Fortunately, we can now cope with the complexities of neural mechanisms underlying cognitive functions without giving up either the principle of integrated action of the nervous system or the cell doctrine.

A DUAL MECHANISM OF LEARNING AND MEMORY

One of the most striking developments in recent years is the accumulation of data in support of the notion, suggested by Hebb (29), that learning is a dual physiological process: (a) an initial and temporary physiological process and (b) a permanent, perhaps anatomical, change, produced by the initial physiological process. A number of rather different experiments bear directly on this two-process mechanism, and they deserve close consideration, for they provide the first real opportunity for analytic dissection of learning and memory and afford an excellent opportunity for unraveling their physiological basis.

Perhaps the clearest evidence comes from the work of Boycott & Young on the octopus (6). These animals can be trained to leave their rocky nests to capture a crab lowered into the water at the far end of an aquarium and to withhold response if a white card is presented with the crab under penalty of electric shock. With six trials a day, half positive and half negative and 2 hr. between trials, the octopuses learned not to attack a crab presented with a white square after one or two shocks, although attacks on the crab alone were quite consistent. Anesthesia, sham operation, and electrical stimulation of the head or the vertical lobe of the brain had no selective effect on avoidance of the negative stimulus, although the latter procedure could produce failure to respond at all for up to three days.

After removal of the vertical lobe, the animals failed to withhold attack on negative trials presented at intervals of 2 hr. or more, and, of course, attacked on positive trials quite consistently. However, if one negative trial followed another within 5 min., then the octopus could withhold attack

successfully on the second trial. Apparently removal of the vertical lobe had destroyed the capacity for "permanent" or long-term memory but had left immediate or recent memory perfectly intact. Similar effects on memory were obtained with lesions of the medial part of the superior frontal lobe or upon sectioning the connections between this lobe and the vertical lobe. The authors suggest that a persistent cycle is set up through the optic, superior frontal, and vertical lobes and back to the optic lobes where a physiological pattern is re-presented over a period of time, possibly long enough to permit growth changes.

This experiment makes two important contributions. First, it is one of the few ablation studies on record that has nearly perfect controls, for the normal performance of the octopus, when tested within 5 min. after punishment for an error, suggests that the lesions produced no defects in perceptual capacity, attention, motivation, etc. Second, this experiment makes a clear and important separation of immediate, very recent, and long-term, permanent memory and, further, points to an anatomical and physiological basis for the distinction.

A second paper, equally striking, is the report of Milner & Penfield (45) on two human epileptics who underwent partial temporal lobectomy including hippocampus, hippocampal gyrus, uncus, and amygdaloid nucleus on the dominant side. In both of these cases, evidence pointed to damage of the opposite hippocampal zone, and both showed severe deficits of recent memory although no impairment of IQ, attention, concentration, reasoning, or skills as measured in a battery of tests. Immediate memory, as in the repetition of digits, was intact, but other tests of memory showed almost total loss after an interval of 5 min. affecting recognition as well as recall, verbal as well as nonverbal memory. A third patient with no damage on the nondominant side, but with the same lesion of the dominant hemisphere, showed no such effects. These two affected cases are quite similar to Scoville's three cases of bilateral temporal lobe damage where recent memory was seriously affected (66).

Here, again, we have a lesion that makes it impossible to retain the effects of learning over any period of time and with no concomitant effects on intelligence, attention, etc. Again a dual process of learning is suggested, for immediate memory is intact. Furthermore, these human cases demonstrate that long-standing memories such as those used in job performance and in taking tests of reasoning, intelligence, etc., are not affected. So perhaps three mechanisms may be distinguished: one for immediate memory, operative perhaps up to five minutes; a second for longer memories on the order of hours or days; and a third, responsible for long-standing memories.

A third experiment of this year fits in very well with these findings and brings to the fore a neglected study almost a decade old. This is the experiment of Thompson & Dean (77) who found, as Duncan (15) had earlier, that the initial physiological process in learning is highly susceptible to electroconvulsive shock (ECS) whereas the more permanent mechanism is not. Thompson & Dean massed the training of experienced rats in a discrimination of

horizontal and vertical striations where the rats had 5 sec. to avoid electric shock by leaving the starting box and then were punished by shock for an incorrect choice. At different intervals following the last criterial trial of a short, massed learning session the animals were subjected to a single electroconvulsive shock, and then after a two-day rest were compared on a re-learning test. The savings of nonconvulsed controls in this study were 80 per cent on trials and 84 per cent on errors; those animals convulsed within 10 sec. after meeting criterion showed savings of only 4 per cent and 1 per cent respectively; those convulsed after 2 min. had savings of 30 per cent and 40 per cent; animals convulsed after 1 hr. had scores of 61 per cent and 63 per cent savings; and those convulsed after 4 hr. were substantially normal (savings of 78 per cent and 88 per cent).

Apparently early electroconvulsive shock can destroy the changes produced by learning, whereas learning and memory are not affected if shock is delayed for some period of time between 1 and 4 hr. Duncan reported the same result in a simple avoidance conditioning situation in which the trials were run one-a-day and the ECS was administered at varying times after each trial. In this experiment one control group received painful electric shock through the feet immediately upon completion of each trial, and these animals were not different in their successful learning from the unshocked and nonconvulsed controls. But the animals that were convulsed within 15 min. after each trial were significantly inferior in learning to avoid. Duplication of Duncan's findings in the hamster have been briefly reported by Gerard (21) who found, further, that keeping hamsters in the cold during the interval between the learning trial and the ECS seemed to lengthen the period of time over which the convulsive treatment was effective in retarding learning.

These experiments have the very great advantage of testing the animals no sooner than 20 or 44 hr. after experimental treatment. Thus, it seems clear that the deficits produced were not the result of defects in perception, motivation, attention, etc., especially since the animals convulsed closest to the test (4 hr. groups) showed no impairment. On the other hand, the question of whether ECS might produce emotional conflict and thus retard learning by punishing the correct response has been raised by Miller & Coons (44). Duncan's control of shocking the animals through the feet after each learning trial, while not perfect, would argue against this interpretation, and so would the evidence on human subjects showing a retrograde amnesia after convulsive treatments and brain concussions, without concomitant emotional conflict. But the possibility cannot be completely ruled out and should be evaluated in future experiments.

At the moment, it appears that these studies do support the evidence for a dual mechanism of learning, and more than that they give us some idea of the time course of the initial process. Apparently, it takes about an hour for the initial memory process to get to the point where it is no longer vulnerable to ECS. With this as a starting point, it would be very valuable to know whether other agents like certain drugs and narcotics would have a similar retarding effect on learning, for not only might we learn more about

the time course of the initial process, but we might also find out something about its physiological and biochemical nature.

We have no idea at the moment of what the nature of the second, permanent process might be except the current suggestion that it may represent anatomical growth. In this connection, it is interesting to note that more and more evidence is accumulating to indicate that certain kinds of new growth are possible within the adult, mammalian central nervous system. In a recent symposium on regeneration in the central nervous system (81) it was pointed out that rich sprouting of axon terminals will occur, following the loss of neighboring terminals as a result of degeneration. We have no idea as yet whether this kind of anatomical change can occur as the result of learning, but this possibility could be investigated experimentally, and certainly those of us interested in the physiological and anatomical basis of learning should watch the developments in this exciting field.

APPROACHES TO THE PHYSIOLOGY OF CONSCIOUSNESS

Much of the recent progress in the physiology of consciousness has been summarized in the symposium *Brain Mechanisms and Consciousness* (12). The psychologist can draw three major conclusions from this symposium. (a) The brainstem reticular formation and the nonspecific, midline thalamic nuclei make up the core of an activating mechanism in the brain which is a multisynaptic system, supplied by all sensory avenues and projecting to the cortex and most of the rest of the brain. Moderate activity in this system leads to fast, asynchronous EEG's and the wakefulness and alert attention commonly associated with this electrophysiological pattern. Inactivity, produced by brainstem lesions or anesthesia, results in slow, synchronous EEG's and unconsciousness or somnolence. Intense activity of the system seems associated with emotional states. (b) This subcortical system, equated by Penfield with his "supracortical integrating mechanism," may play an important role in the organization of skilled, voluntary acts, memory, and intelligence. (c) Unfortunately, most of the conclusions about the role of this system in behavior, apart from certain changes in wakefulness, were based on observations of changes in the EEG (see also 17) or deduced theoretically from behavioral and physiological studies of other parts of the brain.

The few direct behavioral studies reported since the symposium have shown a depression of lever pressing during electrical stimulation of midline thalamic nuclei calculated to "alert" the organism (8) and a reinforcing effect when electrical stimulation of rhinencephalic areas related to the system followed depression of a lever in a Skinner box (57). Only recently have careful observations of behavior been made in chronic waking preparations during direct stimulation of the activating system and simultaneous recording of EEG. Segundo *et al.* (67), working with monkeys, showed the same arousal pattern of fast, asynchronous EEG's when an animal is awakened from sleep by either physiological stimuli or electrical stimulation of parts of the activating system. Substantially the same effects were yielded by stimulating the mesencephalic gray, reticular formation, center median

of the thalamus, hippocampal formation, anterior commissure, and orbital surface, cingulate, temporal and paraoccipital cortices. Weak stimulation of all these points had the effect of awaking the animals and making them responsive to visual and auditory stimuli. Increasing the intensity of stimulation produced "searching behavior," and still further increases in intensity led to crouching and flight. In the animal already awake, stimulation had the curious effect of arresting on-going behavior and then producing the same pattern of "alerting," "searching," and "flight" as a function of increasing intensity. Essentially the same behavioral findings have been observed in other animals like the guinea pig (41), cat (1), and man (28).

Going further, Jansen, Anderson & Kaada (35) found that lesions of the intralaminar nuclei of the thalamus abolished the searching response in cats that could be elicited by stimulation of the medial surface of the prefrontal cortex, cingular gyrus, and medial portion of the hippocampal gyrus. Lesions in the lateral thalamus, hypothalamus, and caudate, however, were ineffective. The data on a few human cases are rather incomplete, but, nevertheless, interesting (28). Stimulation of deeper parts of the brain in waking schizophrenics yields different results dependent upon locus. Stimulating the septum makes patients alert, speak more rapidly, and feel comfortable. Rostral hypothalamic stimulation leads to complaints of discomfort, fullness of the head, and pounding heart; caudal hypothalamic stimulation causes tension and rage, while caudate stimulation produces drowsiness. Stimulation of amygdaloid nucleus results in intense emotion, sometimes rage, sometimes fear, even in the same patient. Hippocampal stimulation produces reports of anxiety. Finally, stimulation of cats and man by intraventricular injection of drugs produces the same kinds of effects on EEG and behavior as the electrical stimulation reported above (18).

Although it is difficult to define and measure consciousness, it is possible to approach the question by determining what stimuli an organism will respond to, behaviorally and neurophysiologically, and thus shed light on awareness or attention and their physiological mechanisms. In a beautifully simple and ingenious experiment, Hernández-Peón, Sherrer & Jouvet (31) recorded the response of the dorsal cochlear nucleus of the waking cat to click stimulation by means of chronically implanted electrodes. Consistent electrical responses to clicks were obtained, but if the cat was distracted by exposing mice in a glass bowl at the time of stimulation, there was no response to the click stimulation in the cochlear nucleus. Similarly distracting the animal with the odor of fish or by mild shock to the forepaw blocked the response of the cochlear nucleus. This is as close as we have come to being able to say neurophysiologically that the cat did not "hear" the auditory stimulus when distracted.

The implication of the brainstem activating mechanism in this depression of the response of the cochlear nucleus in the distracted cat is strongly suggested by the fact that similar depression of response to click stimulation can be produced by electrical stimulation of the reticular formation at the presentation of the click. Furthermore, the habituation of the reticular for-

mation, reported by Sharpless [cited in Jasper *et al.* (36)] and the habituation of related rhinencephalic structures described by Galambos *et al.* (20) lend direct and strong support to the role of the activating mechanism in attention and, therefore, consciousness.

Much speculation in this area, however, still awaits further development of the next step in research in which lesions, stimulation or recording in the heart of the activating system are incorporated into experiments where an analysis of behavioral capacities can be made and correlated with functioning of the brainstem reticular formation and nonspecific thalamic nuclei. We already have many behavioral studies of regions of the nervous system related to the activating mechanism and probably part of it. But at this point it is more conservative and perhaps better to consider them separately and thus avoid unwarranted bias as to the role of this functional system in cognitive processes. In this connection, it is well to remember the caution of Bailey (5) that consciousness is a function of many parts of the brain and that the activating mechanism, important as it is, is only one of the systems at its basis.

ELECTROPHYSIOLOGICAL RECORDING DURING LEARNING AND MEMORY

Working with cats with chronically implanted electrodes, Galambos *et al.* (20) confirmed the earlier report of Hernández-Peón & Sherrer (32) on habituation of the response of the dorsal cochlear nucleus to repeated click stimulation and extended the finding to include habituation of the auditory cortex, hippocampus, septal area, and the head of the caudate from which they also recorded. Once such habituation was established, 10 to 20 randomly selected clicks were paired in a simple conditioning procedure with single electric shocks across the chest with the result that the animals now "alerted" behaviorally to subsequent clicks and showed once again clear-cut responses in the cochlear nucleus and other points recorded in the brain as well. Continued presentation of the clicks with no further electric shocks (extinction) led to another habituation, and further association of clicks with shocks (reconditioning) produced the responsiveness in the system again. Here, then, is a record of the changes taking place in a number of different places in the brain in a simple conditioning and extinction procedure. Two things are worthy of special notice in this study: (a) the electrophysiological changes accompanying conditioning occurred near the origin of the auditory pathway and (b) these changes are duplicated in a number of different loci outside of the auditory system.

One might wonder whether 10 to 20 electric shocks, not paired with clicks, might have restored the responsivity of the habituated animal, and there will be some who will wish to argue whether the procedure employed in this experiment is a typical instance of conditioning. Neither possible criticism is a serious one, however, in light of the fact that this experiment shows beautifully and simply a significant electrophysiological change in many points within the brain occurring concomitantly with consistent modifica-

tions in the animals' responses to environmental stimuli. This experiment is a significant break-through in the physiology of learning, for it provides a new and promising approach, firmly grounded in neurophysiology, in contrast to the many studies of the past that have established a behavioral phenomenon and then searched the broad reaches of the nervous system for a possible neurophysiological basis.

Another investigation of the electrophysiological response of the nervous system in the course of learning is offered in the experiments of Morrell & Jasper (52) and Morrell *et al.* (53). These authors followed, in more detail than others before them, the EEG of animals developing conditioned responses to visual, auditory, and tactile conditioned stimuli in a situation where the unconditioned stimulus was a flickering light. They recorded from frontal, central, parietal, occipital, and temporal regions of the cortex of unanesthetized monkeys by means of insulated steel needles or hooks inserted through the skull. Conditioning, extinction, discrimination, and even spontaneous recovery could be measured with this method. Depending upon the frequency of the flicker (UCS), the conditioned response was either a blocking or desynchronization of the alpha rhythm or a facilitation of the occipital rhythm at 12 to 14 c.p.s. or "photic" driving at the frequency of the flicker (6 to 12 c.p.s.). Before any training, the conditioned stimulus elicited a generalized activation of the cortex; then upon repetition of the CS alone there was adaptation, and this activation disappeared. With the start of pairing of CS and UCS, the first response to the CS was again a generalized activation of the cortex; as pairing proceeded, however, the response to the CS gradually became confined to the occipital area, first as a frequency-specific response, then as a fast, low voltage activation.

Using the same type of conditioning procedure, Morrell and his co-workers (53) then turned to an investigation of the effects of discharging or irritative lesions on the establishment of a conditioned response in monkeys. To achieve a discharging lesion, they made unilateral implants of alumina cream in the frontal cortex, auditory cortex, postcentral leg area, occipital cortex, and amygdaloid-hippocampal region in different animals. The success of the discharging lesions was demonstrated clearly in the EEG records postoperatively. The results showed a marked impairment of conditioning to the CS that involved the sensory area in which the discharging lesion was produced, but conditioning in other modalities was not affected. Much more general impairment, involving all conditioned stimuli, resulted from producing a lesion in the amygdaloid-hippocampal region. But the frontal implant resulted in no effect. Surgical excision of the discharging lesions restored the animals to normal capacity in subsequent tests of conditioning.

Two important points come out of this study. (a) Discharging lesions are much more disruptive of the organization of the nervous system required for learning than simple loss of the tissue through clean, surgical ablation. This conclusion, although contrary to earlier reports (11, 30), finds considerable support in the preliminary study of Harlow *et al.* (26) showing marked effects on problem solution behavior of irritative lesions produced by the implanta-

tion of radioactive cobalt. (b) The amygdaloid-hippocampal region may be critical in the establishment of learning and the maintenance of memories, as has been suspected from cases of amnesia in patients with epileptic foci in this region.

Bearing directly on this last point are the findings of Milner & Penfield (45), cited above, of a loss of recent memory following lesions of this area. Also, the work of Penfield (12), showing the elicitation of memories by stimulation of the temporal lobe of epileptics, and the studies of Pribram and his associates (60, 61, 62) on the role of temporal cortex in discrimination learning all point to the importance of these regions of the brain in the recording of memories and, therefore, the development of a learned response.

A final electrophysiological recording study, related to the role of the amygdala and hippocampus in memory, is the fascinating, but unfortunately incomplete, report of activity recorded in the human brain during recall of emotional memories. Working with three psychotics and one case of intractable pain, Lesse *et al.* (40) recorded from the anterior hypothalamus, amygdala, caudate, rostral hippocampus, septal area, tegmentum, and frontal and parietal cortex. During recall of emotional memories, these subjects showed characteristic bursts of 14 to 17 per second waves from the rostral hippocampus and 20 to 30 per second waves from the amygdala, along with increases in amplitude of records from both loci. Olfactory stimulation and recall of odors gave similar effects in these structures, but emotion or rage elicited during interview yielded nothing consistent. Other points recorded gave no consistent effects, and, furthermore, the effects recorded from hippocampus and amygdala disappeared if current, neutral topics were discussed with these patients after the recall of emotional memories.

SUBCORTICAL STRUCTURES IN LEARNING

The recent development of interest in the role of the activating system and the rhinencephalon in behavior has directed attention away from the neocortex and toward a variety of subcortical structures. Three different kinds of investigation outside of the neocortex will be discussed here: (a) reinforcement through stimulation of the subcortex, (b) effects of subcortical ablations, and (c) spinal conditioning.

Reinforcement through stimulation of the subcortex.—Great interest in the last few years has centered around the possibility of reinforcing the performance of a learned response by stimulating the depths of the brain through chronically implanted electrodes in the waking animal. Such a technique has the obvious advantage of yielding information about the locus of reinforcement processes in the brain and their physiological nature. Thus far, two types of reinforcement have been reported as a result of subcortical stimulation. First, negative reinforcement was described by Delgado *et al.* (14) in cats where the "fear" elicited by stimulation of the region of the spinothalamic tract in the tectum, the lateral nuclear mass of the thalamus, and the inferomedial part of the hippocampal gyrus could serve to motivate escape and avoidance behavior. More recent work by Delgado (13) suggests that

we are dealing here with central mechanisms important in the elaboration of impulses from noxious stimulation.

Perhaps more dramatic, because it involves the technique of self-stimulation and offers one of the few possibilities for getting at "pleasurable" or positive emotions, is the case of positive reinforcement. Confirming Olds & Milner's finding (57) that subcortical stimulation can lead to the reinforcement of lever-pushing, Sidman *et al.* (68) have gone on to show that rats and cats maintained on different schedules of self-stimulation respond much as they do on different schedules of reinforcement with food and water. Variable interval schedules produce much lower rates of responding than fixed-ratio schedules. Furthermore, within small limits, the intensity of the self-stimulation determines the rate of lever pushing.

Stimulation of the septal area, amygdaloid complex, and anterior hypothalamus yields the highest rates of response in the rat while moderate rates were produced on stimulation of the cingulate, hippocampus, posterior hypothalamus, and anterior thalamus (56). In the cat, the caudate seems to be a very effective point of stimulation for positive reinforcement (68). Most of these structures are parts of the rhinencephalic system and some are known to play a role in emotion, but beyond that we have no idea why or how they are effective points in a self-stimulation, reinforcement experiment. The only glimmer of a suggestion we have in the published literature is from the studies of human patients where septal stimulation was said to have produced "comfortable" feelings (28).

At the moment, it is useless to speculate on the nature of this most striking effect of self-stimulation reinforcement when the published facts at hand are so few. One surprising thing perhaps worthy of note is the fact that extinction is so rapid and so complete when the opportunity for self-stimulation is eliminated (57). This means either that self-stimulation is quite different from other kinds of reinforcement or that our concept of extinction is quite limited.

Effects of subcortical ablations.—Recent attempts by Chow (9) and Peters *et al.* (59) to assess the role of thalamic association nuclei in learning and problem solution have yielded negative results. Chow made lesions of the pulvinar and medialis dorsalis of the thalamus because these nuclei project respectively to parieto-temporo-occipital and prefrontal association cortices, known to be important in certain kinds of problem-solution behavior. He reported no effect on various discriminations, delayed reaction, and conditional reaction. Peters *et al.* concentrated on the medialis dorsalis and delayed reaction and likewise found no effect of subcortical lesions. Unfortunately, in both of these cases, the lesions were quite incomplete and bilaterally asymmetrical.

Fortunately, we have information on what can happen with more extensive lesions of the medialis dorsalis, and it is by no means negative. Pechtel *et al.* (58) trained cats in a series of discrimination problems in which they had to learn to choose one of two boxes in response to differential stimuli; next they learned to activate one of these signals by feeding from one of two

cups located on switches, and then choose food boxes appropriately; later they learned to activate switches covered by empty cups; and after that proceeded to even more complicated spatial discriminations. Large bilateral lesions of medialis dorsalis greatly impaired the performance of these simple learned tasks and made relearning much slower and poorer than original learning. Some cats were so badly affected that they could only get as far in the series of problems as depressing the switch if the cup on it contained food. In the first few postoperative days, the cats had to be force-fed, but by the time postoperative testing began they were eating vigorously. Since the operated cats were well motivated and quite capable of performing the simplest portions of the test series, the authors conclude that the defect is not in ability to perform, but that the medialis dorsalis is a thalamic association nucleus of considerable importance in learning and memory. Support for this interpretation is found in the report of Spiegel *et al.* (72) who report that human patients with similar lesions may show pronounced disorientation in time and place and some defects of recent and remote memory.

Spinal conditioning.—The question of spinal conditioning has been raised again with the publication of positive results by Dykman & Shurrager (16) in a very detailed report. These workers studied 15 spinal kittens and one spinal puppy, ranging from two days to three months at the time of transection. Some preparations were acute and tested within a few hours after operation, but many were chronic, maintained for as long as 70 days postoperatively. Transections were made between two ligatures 1 cm. apart between T-5 and L-1. The UCS was a shock to the right hind leg sufficient to give strong flexion of that leg and extension and then flexion of the opposite leg. The CS was the light stroke of a brush on the fur of the right side, the inner side of the left hind leg, or the right side of the tail, none of which evoked movement in the hind legs in repeated tests before conditioning.

Of the 16 animals attempted, some in several different procedures, only three were classified as failures by the authors. Twenty-one attempts to establish successive conditionings and extinctions with the three different CS's gave 12 positive results. In these cases, the criterion was 10 successive CR's in two five-trial units for conditioning and 10 successive failures for extinction. In four to six cycles of conditioning and extinction so run, the mean number of trials for 12 animals to meet criterion dropped steadily from about 30 to 10 for conditioning and from about 40 to about 20 for extinction. In the course of establishment of conditioning, the CR increased steadily in magnitude from a twitch in the right hind leg to repetitive stepping in both hind legs. In general, the latency of the CR was longer (96 msec.) and more variable than the latency of the UCR (28 msec.). Very little spontaneous recovery was observed after extinction, but after conditioning the CR persisted up to 24 hr. Six animals gave positive results in maintained conditioning in which 40 trials per day were given in the establishment of conditioning and extinction. The mean number of CR's in the first seven conditioning sessions of 40 trials each was approximately: 8, 16, 22, 30, 31, 33, 34; over seven extinction sessions the number of CR's was: 28, 25, 17, 14, 13, 6.

In view of the history of claims and counterclaims in the field of spinal conditioning and because of the difficulties inherent in spinal preparations, the positive results reported here must be carefully checked before spinal conditioning can be accepted as a fact. In all fairness, however, it should be pointed out that Dykman & Shurrager have presented a very detailed report of procedure with very striking mean data, covering both acute and chronic animals and successive conditioning and extinction, each maintained over a period of days. In contrast to many other attempts, particularly those with negative results, these workers used very young animals, and found evidence to suggest that the younger the animal the greater the success of conditioning and the greater the ease of conditioning. Also, their use of a brush stroke for the CS eliminates the very pertinent criticism of their earlier use of an electric shock as the CS on the grounds that repeated shocking of a spinal animal may lower skin resistance enough to facilitate reflex hind leg responses to the electrical CS to the point where the CS actually becomes a UCS.

The obvious advantage of a tool like spinal conditioning in the study of the neurophysiology of learning makes it most important to follow up on this report. So far, in all the apt and ingenious criticisms of earlier positive reports of spinal conditioning, no one has come up with any valid reasons why spinal conditioning should not occur under the appropriate experimental conditions.

CORTICAL FUNCTIONS IN LEARNING AND PROBLEM SOLUTION

Investigation of the role of the cortex in learning and problem solution has become more and more sophisticated over the years, with great improvements in testing, better surgery and histology, and better rationale for the placement of lesions. Nevertheless, the principles of cortical function in behavior still remain pretty much of an enigma, with one of the most consistent findings being the surprising capacity of the higher animals and man after cortical insult.

Since this topic has had a number of expert reviews in recent years (25, 76), it will be only briefly reviewed here. This is not to deny the importance of the cortex, however. Despite the current sophisticated tendency to "dethrone" the cortex and the long-overdue investigation of subcortical functions, the fact remains that it is only our ignorance that leaves us with such a scanty list of cortical functions in cognitive processes (10). Even where deficits have been clearly established following cortical lesions, there remain two important questions: (a) what are the physiological principles of cortical functioning, and (b) what are the psychological deficits responsible for impairment of performance following cortical lesions?

Two efforts to get at the physiological principles of cortical function have come out of Sperry's laboratory. One is an attempt to test the "electrical field" theory, reported by Sperry & Miner (70). These workers inserted mica plates into the visual cortex in an effort to insulate one part of the cortical surface from another and found no effect on pattern discrimination. Although this finding fits in with popular bias and with previous failures to

disrupt pattern discrimination by implantation of conducting materials in the cortex (39, 71), all of these experiments have the great weakness that they only infer that cortical fields are disrupted while visual discrimination and perceptual capacity are not.

In a second investigation (54) to get at the basis of transfer of training, cats were trained in a visual pattern discrimination with one eye after section of all the crossed optic fibers, and then were tested with the other eye alone. Transfer was almost perfect in all cases, despite the isolation of the two hemispheres as far as visual input is concerned. However, transfer from one eye to the other fails in these preparations if the posterior 40 to 50 per cent of the corpus callosum is sectioned (55). As much as 70 to 75 per cent of the anterior part of the corpus callosum can be sectioned without impairment. Apparently, then, transcortical fibers through the posterior corpus callosum are responsible for the integration and functional equivalence of the two hemispheres.

A number of excellent attempts to analyze the psychological defects following brain lesions have come from the laboratories of Pribram and Teuber. Two experiments on monkeys show that the severity of visual defects after inferotemporal lesions depends on the difficulty of the task. Whereas post-operative monkeys solve easy simultaneous discriminations normally, they fail if pushed close to threshold (48) or if the discrimination is successive or presented as a conditional reaction (61).

Perhaps the most striking and informative experimental investigation of cortical lesions this year is the very clean-cut differentiation of the roles of different parts of the parieto-temporo-preoccipital association cortex in visual and somesthetic discriminations, made in a study of the monkey by H. Pribram & Barry (60). Parietal-preoccipital lesions had no effect on visual discrimination but markedly impaired initial somesthetic discrimination postoperatively. Inferotemporal lesions resulted in failure on visual discrimination and success on somesthetic tasks. Since the losses here are modality-specific, it can be argued quite effectively that the deficits following cortical lesions are not attributable to a loss of general comparison attitude, motivation, or ability to pay attention. Indeed, the striking thing about this experiment is the remarkable specificity of the loss to modalities, so that if there is a loss of comparison, attention, memory, etc., it most certainly must be modality-specific.

Analysis of the deficit in delayed-response tests following damage to the lateral frontal granular cortex has been made in three experiments by Mishkin & K. Pribram (49, 50, 62). They varied the spatial characteristics of the responses required in delayed alternation and delayed reaction and, at the same time, varied whether the predelay cues were spatial or qualitatively distinctive. The spatial nature of the response is not a factor, for monkeys with frontal lesions do as well in "up-down" tests and "go-no-go" single-stimulus tests as they do in the classical "left-right" tests if given qualitative predelay cues. The frontal preparations fail, however, if required to depend upon spatial predelay cues, but they succeed as well as control operates if

the predelay cues are clearly and consistently qualitatively distinctive. The authors believe that distinctiveness of predelay cues accounts for success of frontal operates reported in other, rather different experiments as well as their own but venture no reason. From a naive viewpoint, one might wonder whether the old and forgotten hypothesis of inattention or distractibility of frontal operates ought to be reconsidered, for it is possible that distinctiveness of cue could compensate for postoperative defects in attention.

Analysis of the defects following brain injury in humans provides much more complex data than experiments on animals, for in this type of study there is no control over the locus and extent of lesions, and, at the same time, it is possible to test for much more subtle and complex defects. Ghent *et al.* (22) studied the learning of tactile discrimination in 36 subjects with unilateral, penetrating injuries of the brain. They found that regardless of the locus of the lesions and regardless of whether or not there were sensory and motor defects in the hand tested, the hand contralateral to the injury was inferior to the ipsilateral hand. Interpretation of this straightforward result is complicated by the similar experiments of Weinstein (78, 79, 80) on weight and tactile size judgments in a similar group of brain-injured humans. First, he found that brain injury disturbed tactile size discrimination much less than weight discrimination, and then only if there were somatosensory defects in the hands involved. Second, in an analysis of time errors made in tactile size and weight judgments, he found greater negative time errors for unilateral than for bilateral testing, but this time the presence of somatosensory symptoms in the hands made no difference. In tactile size the exception to this generalization was that those with frontal injury had the greater time error for bilateral stimulation; nonfrontals and controls were indistinguishable. In weight judgments time errors with the affected hand were the same for all groups, but the brain-injured showed significantly greater time errors than the controls with the normal (ipsilateral?) hand. Those with parietal lobe injury showed the greatest negative time error, and those with frontal injury showed the least.

So far, little insight has been gained into this constellation of deficits resulting from these extremely detailed and sensitive comparisons of the various brain-injured groups and controls. Of course, tests of sensory learning and of perceptual capacity with different modalities can be expected to yield different results, and it is obvious that more of these careful experiments are needed before a coherent interpretation can be made. At this point, however, one has the feeling that our ability to investigate pertinent variables experimentally and to analyze statistically significant deficits associated with these variables far outstrips our comprehension of the neurophysiological organization of the central nervous system. It can be hoped, with good reason, that such excellent behavioral analysis of the performance of the brain-injured may help to point the way to better understanding of neural organization.

SUMMARY OF PROGRESS IN THE PHYSIOLOGY OF COGNITIVE
FUNCTIONS

Assessment of the year's progress in such a variety of approaches to the neural basis of cognitive functions is a most difficult task at this stage of knowledge. There is no one best approach and no one best theoretical viewpoint. In fact, it would be a mark of progress to recognize that we are dealing with a multitude of processes in the investigation of cognitive functions and that each psychological subgrouping, like learning, memory, or consciousness, is itself a complex of processes, different facets of which are measured in different experiments. In this light, the variety of current methodologies and conceptualizations is most healthy, for it seems true that the significant break-throughs can still come where least expected.

The most encouraging developments of the last year are as follows: (a) the analysis of the mechanisms of learning and memory into a series of different physiological processes, organized in time, (b) the implication of the amygdaloid-hippocampal region as one critical focus for learning and memory, (c) the recognition, nevertheless, that the changes in learning and memory occur in many places in the brain simultaneously, (d) appreciation of the significance of the subcortex in cognitive functions, particularly the complex integrative role of the reticular and thalamic activating mechanism in consciousness and attention, and (e) the successful use of Hess's implanted electrode technique for stimulation and recording in the chronic, waking preparation and the experimental production of discharging brain lesions as additions to the classical ablation method.

Many difficulties remain, and these can be listed, too. (a) We must clarify behavioral concepts and develop new and more powerful methods for their experimental analysis particularly if we are to gain full advantage of the tremendous development of technique, fact, and concept that is taking place in neurophysiology. (b) We should not be uncritical in our enthusiasm for the potential role of the subcortical activating mechanism in complex behavior, for there is the danger it will become the theoretical wastebasket that the cortex has been, the repository of mentalistic concepts and unsolved experimental problems.

MOTIVATION

To review the highlights of progress in this broad field briefly and yet with any adequacy, it will be necessary to confine the topics covered, and the areas of hunger and thirst have been chosen, for, in the reviewer's estimation, the most striking progress has been made here.

Thirst.—A series of papers by Andersson & McCann (2, 3, 4) has confirmed and extended the senior author's pioneering investigation of hypothalamic mechanism in thirst. Working with water-satiated goats, they found that repeated electrical stimulation as well as single injections of hypertonic solutions into the hypothalamus promptly produced great drinking. The critical area for injections is dorsal to the infundibulum, between the

fornix and the tract of Vicq d'Azyr, just lateral to the paraventricular nucleus, and midway between the dorsal and ventral hypothalamus. Electrical stimulation was effective over a smaller dorsal and central core of this area, and although drinking could be repeatedly elicited with this method, it is significant that the drinking only outlasted the stimulation by a matter of seconds. Similar results have also been obtained in one rat by electrical stimulation of roughly the same part of the hypothalamus, yielding water intakes up to 400 cc. in 24 hr. upon repeated stimulations (23).

Turning to the dog and the ablation method, Andersson & McCann (4) found that lesions of the same hypothalamic area resulted in total or very great reduction of water intake, despite normal food intake and the accumulation of severe dehydration. Interestingly enough, the adipsic dogs would drink milk or broth and thus could be kept alive. As in the case of rats that refuse food after hypothalamic lesions (75), these adipsic dogs eventually recover and resume normal ingestion, in this case within 14 days.

The hypothesis that thirst is controlled by specialized osmoreceptors in the hypothalamus receives considerable support from these experiments, especially those using hypertonic solutions. It should be pointed out, however, that stimulation with hypertonic solutions is not necessarily confined to the possible osmoreceptors concerned with thirst, for Larsson (37) has reported the elicitation of eating, rumination, chewing, and licking upon injection of hypertonic solutions into the hypothalamus of goats. Furthermore, it should be kept clearly in mind that the dramatic effects produced by central stimulation and ablation do not lessen the importance of humoral factors and peripheral neural factors in thirst. For example, Montgomery & Holmes (51) have shown that gastric distension is an important variable in the inhibition of drinking. They introduced water into the stomach or inflated a balloon in it and depressed drinking induced by salt injection if the distension was allowed to operate for at least 20 min. before the opportunity to drink. Cocainizing the stomach can eliminate this inhibition. The long distension required in this study suggests the effect may be humorally mediated.

Hunger.—Much of the progress that has been made in the investigation of the physiological basis of hunger in recent years has been summarized in the New York Academy of Sciences symposium "The Regulation of Hunger and Appetite" (47). One of the most interesting advances reported in that symposium is the use of peripheral arteriovenous or capillary-venous glucose differences as an index of hunger. Reflecting the rate of glucose utilization, capillary-venous glucose differences decrease when subjects report hunger and increase when they are satiated. Furthermore, a recently purified hyperglycemic factor, glucagon, which increases capillary-venous differences and blood glucose values, abolished gastric contractions and decreased the experience of hunger in human subjects (73). These findings seem to fit with Mayer's (47) glucostatic theory of hunger which says that hunger and satiation are determined by the activity of glucoreceptors in the hypothalamus which are sensitive to changes in blood sugar and in the utilization of glucose. Unfortunately, the evidence bearing on this very attractive theory is by no means firm, and it still must be regarded as only a plausible inference

from the data on the role of carbohydrate metabolism in hunger and the rather clear-cut importance of the hypothalamus.

A number of studies of the mouse have extended our knowledge of hypothalamic function in hunger, for it has been shown that ventromedial lesions will produce hyperphagia in the mouse (43) and that the hyperphagia produced by goldthioglucose injections in the mouse is associated with degeneration of cells in the ventral hypothalamus (42). Perhaps the most informative of the recent studies of hypothalamic control of hunger is the report of Teitelbaum (74) that hypothalamic lesions have some of their effect by changing the animal's sensitivity to the stimuli provided by food. Ventromedial lesions produce "finickiness" as well as overeating, for hyperphagic rats refuse to eat powdered foods adulterated by nonnutritive cellulose and quinine at levels that do not affect normal rats. Furthermore, the lesions increase the overeating the hyperphagics do much more than normals if offered hard food or powdered food mixed with 50 per cent dextrose. Some of these results may be a result of the obesity itself, for hyperphagics kept from becoming obese by restricted feeding show only the sensitivity to the addition of nonnutritive cellulose. Whether this result occurs because of the enormous, and perhaps heedless, eating of the nonobese hyperphagics or whether it is the result of a change in the internal environment caused by obesity remains a question. It is worth noting that very similar results have been obtained with cellulose and quinine adulterations of the diet in the genetically obese mouse (19).

Investigating the hypothalamic control of eating with implanted electrode techniques, Smith (69) found that electric stimulation of the ventromedial hypothalamus caused hungry rats to reduce their food intake greatly. Conversely, electrical stimulation of the excitatory mechanism for hunger in the lateral hypothalamus greatly increased food intake. In both of these cases, the changes in food intake appeared rapidly after the onset of stimulation, and then, in contrast to what happened in the thirst studies of Andersson & McCann (3), persisted long after the brief period of stimulation was over. It is interesting to note that, in this case, the effect of electrical stimulation was the simple converse of the effect produced by ablation of these same structures.

PSYCHOPHARMACOLOGY

The recent great interest in the use of "tranquilizing" drugs in the management of mental patients has directed an enormous amount of attention to the question of the effect of drugs on behavior (34). The scope of the problem, behaviorally and neurophysiologically, and hope for practical application that the tranquilizers have inspired can be seen from the recent New York Academy of Sciences symposium "Reserpine in the Treatment of Neuropsychiatric, Neurological, and Related Clinical Problems" (46). Reserpine, and now numerous related compounds, have been studied in investigations of reflex functions, synaptic phenomena, EEG, animal learning, and the management of psychotics, mental defectives, behavior-problem children, and the aged as well as for the control of enuresis, infant colic, headache, convulsions, asthma, dermatitis, and fear before surgery. Despite the use

of the "double-blind" method, in which neither the doctors nor the patients know who gets the drug or the placebo furnished by the drug companies, most of these studies are poorly controlled and poorly executed and, in addition, claim positive results.

At present, it is clear that many of the tranquilizing drugs have powerful effects on behavior, particularly the effect of reducing activity without loss of consciousness. But there are three scientific questions which have not yet been answered satisfactorily. The most empirical and practical question is: do the tranquilizers produce any practically and statistically significant effect on behavior in general and mental disturbance in particular? The second question is: where do these drugs act in the nervous system and what kinds of neurophysiological effects do they produce? The third, and scientifically most basic, question is: how do these drugs exert their effects, through what effect on the biochemistry of the nervous system?

There is not space to go into the tremendous data bearing on each of these points. Instead a brief summary of certain selected evidence will be offered.

The problem of validating a new psychiatric treatment is enormous as recent history has shown. In the present case, most investigators do not ask whether the tranquilizers "cure" mental disturbance but rather whether the drugs make the patients more manageable and more accessible to other types of treatment. Even so, the problems are great as evidenced in the excellent methodological study of Hall & Dunlap (24). These workers studied 175 patients by the double-blind method and found significant improvement in the drug-treated group on many tests and items of behavior rated. They also noted, however, that the controls showed a surprising degree of improvement, often not mentioned in other studies, and raised the question of positive effects attributable to factors other than the drug. Then they went one step further and asked all the testers, raters, and interviewers to guess which patients received the drug and which received the placebo. The guesses were considerably above chance, because of the fact that the patients receiving drugs showed numerous recognizable side effects like mild symptoms of Parkinsonism, for example. One wonders how much of the results of other experiments can be accounted for by factors like these, and, therefore, how solid our scientific assessment of the value of tranquilizers is.

On the question of the neurophysiological effects of the tranquilizers, many careful experiments have been performed. The data available suggest that reserpine may have the effect of blocking afferent impulses to sympathetic centers in the diencephalon (27, 65) and that both reserpine and chlorpromazine can depress parts of the reticular activating system (33, 63, 64).

From the biochemical point of view, the attempt has been made to understand the action of the tranquilizers in terms of their possible antagonistic effect on certain cerebral chemicals like serotonin, that some think might be responsible for mental disturbance if they occur in excess amounts for any reason (7, 82). Taken as support for this belief is the evidence that some of the hallucinogens like lysergic acid diethylamide may have the converse

effect of blocking the metabolism of serotonin, thus leaving an excess (34). While this simplified picture is quite speculative and far from proven, it is nevertheless worth noting that much excellent research on the role of cerebral chemistry in mental disorder is being provoked by the current interest in tranquilizing agents.

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SPECIAL DISABILITIES¹

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Somatopsychology is the study of the ways in which physique may influence behavior by mediating the effectiveness of the body as a tool for actions or by serving as a stimulus to the self and others. A field with a long past and a short history, it furnishes the framework in which special disabilities are here discussed.

From ancient times to the present day thoughtful observers have remarked on variation in behavior as a function of physique, especially atypical physique. The birth and early growth of clinical psychology were based, in part, on studies by Itard, Seguin, Binet, and Witmer of children with physical handicaps. The interest of psychologists in the psychological effects of physical variation has not been constant, however, nor has the field grown in the same ratio as have some other branches of psychology. There was a brief spurt of interest after World War I, but it was not maintained. The present strong development of somatopsychology, dating from the period following World War II, shows a vigorous and flourishing literature in both basic and applied work. In great part, this literature reflects increased general interest, both public and private, in practical problems of rehabilitating the disabled. However, there is also clearer recognition of the field as important to psychology as a science.

Publication problems.—The literature of somatopsychology is widely scattered. *Rehabilitation Literature* (74), which maintains the most comprehensive abstracting service in this field, receives more than 600 periodicals. Unlike child, animal, and other fields of psychology in which attention is centered on a particular class of behaving subjects, there is no single publication devoted to the psychological aspects of physique and disability. There are scientific journals devoted to specific disabilities such as speech and hearing problems, amputations, and tuberculosis, and almost every disability group has a voluntary health organization which maintains some kind of publication. Studies of somatopsychological interest appear to be published more frequently by these medical, educational, and voluntary health organization periodicals than by psychological journals. This is an unhealthy situation for several reasons: (a) Diversification of publication outlets does not seem to mean that disabled subjects are used in a wide variety of fundamental investigations. Rather, it contributes to stimulus-bound investigations in which specific disabilities are assumed to be psychologically meaningful in themselves. (b) Practically, the lack of a journal in somatopsychol-

¹ This review covers the four-year period beginning about May 1, 1952 and ending May 30, 1956. It was prepared, in part, while the author was a Special Research Fellow of the Institute of Mental Health, Public Health Service.

ogy means that many excellent research studies never reach print, for there is no suitable outlet for them. They are too complex for the magazines of voluntary health organizations, and too long or lacking in general interest for psychological journals. This is particularly true of studies prepared as doctoral theses. (c) Some research reports may appear as brief articles or as abstracts. Judging from the number of papers rejected for this review because reporting was inadequate for understanding or independent evaluation of data, it is not clear that brief publication leads to optimal assimilation of knowledge. Brevity may be the soul of wit, but it is not the soul of science. In the reviewer's opinion, serving the informational needs of the general reader or practitioner is not the major function of scientific publication. Providing a firm basis for the evaluation of research leading to the reconciliation of differing results and the growth of knowledge is. Editorial need for short manuscripts is understandable, but it is not necessarily praiseworthy. To the extent that brevity inherently discourages and inhibits critical appraisal of research, and to the extent that it encourages the circulation of conclusions which rest on research errors that cannot be detected, brief publication may be considered malignant. In young and poorly developed fields such as somatopsychology, publication restrictions upon the dissemination of knowledge may be fatal to scientific development.

A published fragment of a study by Dembo *et al.* (26) may serve as an example of publication atrophy. The original research (25), in the reviewer's opinion, is one of the most important somatopsychological investigations that have ever been made. Its findings and its theoretical contributions have had great influence upon the few who have been fortunate enough to see the full mimeographed report, and part of its value is being communicated through the writings of others. The original report, however, has not yet been published.

Scope of the review.—In 1952 *Rehabilitation Literature* (74) abstracted 1065 articles and books. There has been a steady growth each year so that at the present rate more than 1500 abstracts will appear in 1956. *Psychological Abstracts* (71) shows a similar rate of growth in the items included under the primary heading, "Physical Handicaps." About 100 items were abstracted in 1952 and 150 items in 1955. This may be compared with about 600 items that were abstracted in 1955 under the heading "Childhood and Adolescence."

Not all of the material relevant to somatopsychology appears under the "Physical Handicaps" heading. A count of the number of abstracts indexed separately for 12 of the most common of the 35 separate diseases listed in *Psychological Abstracts* for 1955 totaled more than 200 items. It is only a slight exaggeration to say that every disease, except the common cold, and every disability, from blindness to dental caries, has a psychological literature.

From this wealth of material, parts of the following areas have been selected for review: general works or reviews that are relevant to more than

one disability, blindness and impaired vision, deafness and impaired hearing, and crippling. Other equally important areas such as illness and hospitalization, normal variation in physique, employment of the disabled, international developments, and professional problems in rehabilitation, have had to be omitted. Seidenfeld's (78) review considers some of these areas.

For each section there was a wide range of reports from which to select. Preference was given to studies presenting experimental data on new or important problems. Previous reviews of the literature and the considered evaluations of the reviewers are included. Clinical, therapeutic, educational, and psychosomatic investigations; general discussions; case studies; personal documents; unpublished doctoral dissertations; unsystematic observations and experiences; and reports of medical, educational, or social work techniques were generally omitted.

In many respects this is an unfortunate limitation. There is some excellent work in recent theses that has not been published. In addition, there is far more experiential and intuitive understanding and misunderstanding of the psychological effects of illness and disability than has yet been confirmed by scientific research. Many important problems have not been studied. Many others have not as yet been stated in forms that are amenable to investigation.

The popular and didactic writings of psychologists, psychiatrists, social workers, and disabled autobiographers often seem rich in both insightful leads for investigation and in nonsense. Sometimes these insights have high face validity and are widely honored. They gain acceptance by consensus and result directly in changes in practice without benefit of experimental study. More frequently insights are only partial and conflict with other partial insights. The result is heated controversy without the experimental research that might lead to resolution. For example, it took 20 years for Cutsforth's (24) insights on blindness to be appreciated and contrary notions to be forgotten. On the other hand, somatopsychological research, hindered by conceptual difficulties and only partially adequate methods and an insufficient supply of able investigators, often seems able to cope only with peripheral, psychologically insignificant problems. Progress is slow.

OVERVIEW

Consolidation of past gains.—The past four years have been a period of consolidation and growth. It has become increasingly clear that physique per se is not central to any psychological variable and that disability directly coerces only physical behavior. Like animal and child psychology, somatopsychology takes as a starting point a particular physical class of subjects. Before psychological meaning can be assigned to obtained findings it is necessary to invent psychological constructs which explain the process by which physique may influence social and psychological behavior. The prevailing view today is that this influence is exerted primarily through the psychological situation that physique helps to create for the person.

Another area of consolidation is the increasing understanding of a reciprocal relation between progress in somatopsychology and progress in other areas of psychology. In the field of mental disease psychologists have long recognized that the study of the ill and malfunctioning is often a royal road to the understanding of the normal and well-functioning. To the degree that atypical physique may result in exposure to severe stress in naturally occurring situations temporally extended far beyond the limits that may be imposed in the laboratory, it is evident that work with the disabled can contribute valuable information to the study of normal processes. Similarly, gains in psychological understanding obtained from work on normal subjects may frequently be generalized to the disabled.

A third area of consolidation reflects the narrowing of the gap among organic, psychosomatic, and somatopsychologic approaches to illness. There are continuing efforts to determine what kind of person gets what disease. Fisher & Cleveland (35), who interpret Rorschach protocols in terms of body image, have been among the more successful in differentiating psychosomatic from organic illness. There is increasing recognition, however, that efforts to distinguish the personality patterns of people who succumb to different diseases have been largely unsuccessful. Waxenberg (84) authored one of the more recent investigations which showed no differences, on a conventional test battery, between psychosomatically and nonpsychosomatically ill women.

There is considerable agreement that all diseases are psychosomatic in some degree, all diseases are organic in the sense that physical or physiological changes must be present, and all diseases that result in stress via their negative personal or social meanings have similar somatopsychological effects. Once illness has been established, psychological tests usually yield a common pattern. The content of the problems created by different illnesses and disabilities may differ, but the psychological dynamics do not. The effect of recognized disease on the psyche is a common effect.

GENERAL WORKS

Reviews and texts.—Garrett (43) edited a useful review of somatopsychology for rehabilitation workers. Bellak (11) compiled a more didactic treatment written by and for physicians. Kirk (55) edited a strongly research-oriented review of the education of exceptional children.

A revised edition of a literature review, integration, and field-theoretical analysis of disability as a social psychological problem by Barker *et al.* (6) appeared in 1953. This seems to be the most frequently cited single publication in the field, and it is the basic reference point for the present review.

On the textbook level, Cruickshank (22) edited a volume that clearly indicates the coming-of-age of the psychology of exceptional children as a scientific discipline. Unlike some past and present textbooks with similar titles (4, 40, 56) which are largely discussions of disease, physiology, and educational administration, or primarily concerned with intelligence and

mental deficiency (44), this volume is a rigorous and comprehensive treatment of exceptionality as a field of psychology.

A deliberate search by the reviewer, in Europe, for European contributions, was unrewarding. A new edition of Hanselmann's (49) book appeared in 1953, but it seems to be unchanged from the 1932 edition. It is an excellent source, however, for earlier European work.

Recent gains.—The outstanding event of the period under review has been the publication of a book by Barker & H. Wright (7) describing the psychological living conditions and behavior of a few children with physical disabilities. This must be considered a major methodological break-through. It is a common observation that disabled children may often live in different psychological environments and behave differently than others. In the absence of effective tools for describing environments and behavior, however, it has never been possible to specify these differences with precision. Barker & Wright were primarily interested in the psychological ecology of children in general. They used disabled children mainly as foils for testing the validity of their instruments by contrasting the situations of disabled and nondisabled subjects. Their findings on disabled children, however, are no less useful than if knowledge of the disabled had been their main objective.

Some important findings were as follows: (a) Behavioral variety for institutionalized disabled children was greatly restricted. The institution provided 57 behavior settings compared with 2030 settings for children living in their own homes in a small town. (b) Responsible participation within the behavior settings, a measure of richness of life for individuals was low for the institutionalized. (c) The town children had higher status. They also showed an increase in the range of behavior settings open to them and the degree to which different settings could be penetrated with increase in age. There was no similar increase with age for the disabled children. (d) Nutrition, physical health, and personal appearance were the primary action patterns for the disabled children. For the town children these were minor, and sociability, play, work, and education were primary. (e) There was no evidence that disabled children differed greatly in behavior structure. The outcome of behavior episodes with respect to success and failure, frustration and gratification, and general tension level was virtually unrelated to motor and intellectual abilities. The authors conclude from the last finding that the structure of behavior is centrally, not peripherally, determined. If this is so, it may account for the lack of psychological disturbance in children who are not externally coerced.

This report is rich, too, in data on parent-child and child-child relationships in disabled and nondisabled children. Its major value, however, lies not in its specific findings but in its methodology and conceptualizations which provide a feasible means for the discovery and analysis of the relevant loci of behavior.

The contributions of Dembo *et al.* (26) are based upon the conceptualization of misfortune as a psychological event. They describe in detail the social-

emotional relationships between fortunate and unfortunate individuals that arise as a consequence of perceiving oneself or others to have suffered a misfortune. In brilliant, systematic exposition, they show that it is not the objective physical loss from which an ill or disabled person suffers but the value system of the judge who assigns or withholds devaluation of the "total person."

Adjustment, or "acceptance of loss," is conceptualized as a process of value change. Adjustment is not facilitated by realistic attempts to maintain the standards and values of the noninjured. Attempts to equal or surpass the nondisabled in certain performances are not adjustive even when attempts are partly successful. Adjustment is not facilitated either by unrealistic actions such as denial of the injury, trying to forget it, or behaving "as if" it were not there. Adjustment to disability does occur, both in disabled and nondisabled persons, when the person with a variation in physique is not perceived as an unfortunate person.

Perception of a disability does not simultaneously require the devaluation of the person who has it. This is the psychological meaning of that vague and much abused phrase "acceptance of disability." It is the perception and acknowledgement of limited, finite loss without the connotation of "unfortunate person." This kind of acceptance often requires a change in the value system of the perceiver.

Some of the changes required are the following: enlargement of the scope of values so that a single value is not seen to be decisive for judgment of a multi-valued person; perception of the body as a possession similar to other material possessions rather than a personal characteristic of the owner; and perception of assets as good and valuable things in themselves which do not require comparison with the greater or lesser assets of others.

Dembo and associates in their original work (25) also made valuable contributions, incompletely covered in the published fragment (26), to the understanding of "mourning," the nature of sympathy, and the dynamics of values.

Barker & B. Wright (5) spelled out some additional implications of the misfortune-devaluation hypothesis. They considered also the physical, social, and emotional insecurities that are associated with physical disability and suggested some important ways in which security may be fostered.

Meyerson (61) hypothesized that both disability and handicap are socially and culturally determined value judgments which are only indirectly related to physique. He summed up the process of adjustment as follows: (a) No variation in physique requires psychological maladjustment. (b) If an emotional handicap exists in a person who has a physical disability, it does not stem directly from the variation in physique but has been mediated by social variables. (c) The mediation between physical status and psychological status occurs in the following way: (i) The person lacks a tool that is required for behavior in the culture in which he lives, and he knows that he lacks it. (ii) Other individuals perceive that he lacks an important tool and

devalue him for his lack. (iii) The person accepts the judgment of others that he is less worthy (or, to the degree that he is a product of his own culture, he judges himself as less worthy) and devalues himself. The sequence is a unit. If (i) or (ii) does not occur (iii) does not occur. If (iii) does not occur there is no psychological maladjustment.

These four theoretically oriented reports have in common the notion that physique and disability as physical variables are not directly psychologically meaningful. Before understanding, prediction, and control of somatopsychological phenomena are possible, it is necessary to transform the phenotypic data of physical variation to genotypic psychological constructs. The resulting genotypic constructs are then generalizable to a wide range of apparently divergent phenomena.

The formulations of Barker & H. Wright (7) which deal with the loci of behavior are clearly in the main stream of field-theoretical social psychology (29). The formulations of Dembo *et al.* (26) which deal with the loci of evaluation are clearly pertinent and reinforcing to the main current of Rogerian clinical psychology (75). This is evidence that somatopsychology is not merely a field of applied work but may be a fundamental area of psychological science.

Areas in which further gains are possible.—Attention may be called to several publications which offer stimulating leads for future work.

(a) Adlerian individual psychology. Adler's formulations of the effects of organic inferiority have seemed convincing to many, but they have not led to fruitful experimentation. Part of the difficulty has been the lack of systemization in Adler's writings. This obstacle has now been overcome with the appearance of the Ansbachers' (3) beautifully organized volume.

(b) Effects of deprivations in infancy. The behavior of many individuals handicapped from infancy or early childhood seems remarkably similar, in some respects, to the behavior of other kinds of maternally deprived, isolated or experientially restricted organisms. Bowlby's (15) summary of human studies, the Beach & Jaynes (10) summary of animal studies, and individual experiments such as those of Weininger (86) offer exciting vistas of important research that can be extended to somatopsychology with reciprocal benefits to general psychology.

(c) Chronic illness. The report edited by Harrower (50) and Zborowski's (96) pioneering study of cultural components in response to pain are rich in insights and problem formulations. With few exceptions, however, almost all present reports of the behavior of the chronically ill are impressionistic. The plausibility and insightfulness of these reports are relatively unsatisfactory substitutes for the research skills and the data that psychologists can contribute.

(d) Effects of institutionalization. The integration of handicapped children in regular schools is presently an area of great activity. This movement seems to be proceeding almost entirely on emotional and pragmatic bases without provision for rigorous study of effects and consequences.

Frampton & Kerney (39) give a brief history of the controversy that has raged for more than 100 years between advocates of residential schools and day schools. Fiedler (34) is one of the few who present data. It seems time to reformulate this problem in terms of what kind of schooling, under what conditions, results in what effects, upon whom. Tools and techniques for this task are readily available.

BLINDNESS AND IMPAIRED VISION

An excellent 4200-item annotated bibliography of the educational, psychological, and social literature relating to the blind has been prepared by Lende (56). Recent reviews of the psychological literature up to 1953 have been made by Barker *et al.* (6), Lowenfeld (59), Meyerson (61), and Raskin & Weller (73). Lowenfeld's treatment is the most comprehensive with respect to time span and area covered. The Barker *et al.* volume is the most pertinent for social psychologists. Both treatments are rich in suggestions for further research. Raskin & Weller surveyed investigations in progress. Most present studies are in the area of personal adjustment to blindness and psychological development. Few investigations are concerned with testing, education, or vocational experience.

Travel skills and orientation.—One of the most outstanding achievements of the period under review has been the ending of more than 200 years of argument, speculation, anecdotal reports, and methodologically deficient research concerning "facial vision" or the ability of the blind to perceive obstacles. Earlier work on this problem has been summarized elsewhere (61).

Under controlled laboratory conditions, the following findings were obtained: (a) auditory stimulation was the necessary and sufficient condition for the perception of obstacles by the blind. Deaf and deaf-blind subjects did not have "obstacle sense" and were incapable of learning it. (b) Pitch or, more precisely, the Doppler effect and not loudness or simple echo was probably the auditory dimension that is critical. (c) At speeds of normal walking, the perception of frequencies of 10,000 c.p.s. and above was necessary for obstacle perception; the perception of frequencies below 8000 c.p.s. was insufficient.

Two problems were still unsolved at this point. One was related to the necessary and sufficient conditions for obstacle perception under conditions of daily life. The other was the relation of wave length and velocity of movement to the distance at which obstacles could be perceived. The first of these problems has now been resolved, and progress has been made on the second.

Ammons *et al.* (2) using 20 normally seeing but blindfolded undergraduate students found that, under nonlaboratory conditions where cues other than audition could be utilized, no single condition was necessary for obstacle perception. Audition was the most reliable, accurate, and universal of the various cues. When auditory cues were not available, however, blind and blindfolded subjects could and did use any and every cue that served them—cutaneous, thermal, olfactory, or cognitive.

McCarty & Worchel (60) used the naturally occurring phenomenon of a

bicycle-riding blind boy to cast light on the reality of the Doppler shift as one means of obstacle perception. They had the boy ride his bicycle with varying speeds along an obstacle-studded path. The experimental design was not flexible enough to measure the upper limit of the boy's ability to perceive and avoid obstacles. However, it was amply clear that increase in speed did not increase the number of collisions. This may be interpreted as additional tentative support for the theory of perception based on the Doppler effect.

Of equal importance, McCarty & Worchel described in some detail the system of object location that the blind boy had developed using his own ability to make a clicking sound with his mouth. This self-generated sound source, coupled with the boy's ability to use different dimensions of the same signal to obtain different kinds of information about his environment, has numerous advantages. It suggests that equal attention to the physiological capacities and the learning abilities of the blind may be of equal or greater fruitfulness than the present extensive and continuing search for mechanical guidance devices.

These studies have immediate and practical significance. It has been demonstrated that obstacle perception can be learned and that the relevant variables are understood. Although there are undoubtedly other reasons for the relative immobility of blind individuals, lack of obstacle sense needs no longer to be one of them.

In another investigation, Bitterman & Worchel (14) reported that blind and sighted subjects in an upright position were equally well oriented to the vertical and the horizontal. When the body was tilted, however, greater disorientation was shown by the sighted subjects. The results were interpreted as evidence for the dominance of vision in the orientation of visually normal individuals. This research is an example of one of the ways in which the study of the disabled can contribute to the solution of important, general psychological problems.

Personality.—Several documented studies of personality have appeared. Bauman (9) made an intensive, large scale study of adjustment to blindness and reported her findings in monographic detail. Using a variety of testing instruments and a structured 126-item interview schedule with 443 blind adults, she compared employed blind individuals who were considered well adjusted with blind individuals who were unemployed and poorly adjusted. Her findings show that the former group was significantly superior to the latter in measured intelligence, manipulative ability, and personality inventory scores. The groups overlapped considerably, however, and a causal relationship was not shown.

Fitting (37) made a rigorous attempt to develop an inventory-type test of personality specifically for blind adults. His basic assumption was that attitudes toward problems of blindness could be used as a measure of personal life adjustment. The resulting measure was adequately reliable and internally consistent, but results from it did not correlate highly with ratings of adjustment given by instructors at adjustment centers for the blind.

The use of inventories devised and standardized on a blind population

and the detailed reporting of findings are important advances in this field. The value of both studies, however, is limited by the known limitations of personality schedules generally (31) and by the difficulty of determining to what degree a client is revealing himself as compared to the degree that he is simply reporting the reality aspects of his situation.

A large number of questionnaire, survey, and enumerative studies have appeared. These descriptive reports serve a valuable "public-health" function. In the absence of experimentation on critical problems, they assess the present conditions of life for the blind and they serve as common-sense guides to conditions that should be changed or improved. None of them may be considered rigorous, however, in the sense that there is justification for generalization beyond the particular sample that was studied. In no case were causal relationships demonstrated.

Buell (17) and Fitting (38), for example, surveyed the present rehabilitation status of students formerly at schools for the blind. Raskin (72) pointed out some of the limitations of vocational counseling services and the nature of the disputes between schoolmen and rehabilitation personnel. Gravitz (45) reported that a sample of blind adults was similar to the sighted in amount of social participation. Thiele (83) reports some relationships between rated personal characteristics and occupational achievement.

Barry & Marshall (8), Hallenbeck (47), and Elonen (32) reached similar conclusions concerning the relationship between behavioral disorders and child blindness from retrolental fibroplasia. They are agreed that much, if not all, of the disturbance found in their subjects was not a function of blindness but of disturbed interpersonal relationships, particularly with the mother. It is beginning to be seen that the behavior of disturbed blind children is remarkably similar to the behavior that has been reported for other kinds of maternally deprived children (16) and can be understood in terms of the same psychological concepts.

Psychologists who, for purposes of illustration, have used Helen Keller's earlier accounts of her development will welcome her new description (54) of her relationship with her teacher, Anne Sullivan Macy.

Impaired vision.—Impaired vision has been extensively treated as a medical and educational problem (51). Although it seems reasonable to believe that visual limitations should have educational and psychological effects, present interest in this field is restricted to ophthalmologists, optometrists, and educators. There has been no evidence in the past and there is no evidence now that normal variations in vision have any significance other than physiological (61).

DEAFNESS AND IMPAIRED HEARING

Deafness.—A comprehensive, 10,000-item bibliography of the literature on hearing and deafness, including psychological aspects, has been compiled by Loring (58). The psychological literature up to 1953 has been reviewed by Barker *et al.* (6), Meyerson (62), and Di Carlo & Amster (30).

Barker *et al.* place great emphasis upon the methodological and technical limitations of much of the experimental work that has been done. They show that few of the obtained findings can be taken at face value. The inconclusiveness of the experimental researches is ascribed in part to the inadequate or improper use of psychological measures and in part to confusing physical and psychological variables. They believe that progress in somatopsychology depends on discovering the mediating psychological variables between sensory disorders as physical facts and as sources of behavior. Some of the psychological concepts that seem fruitful in dynamically relating physical characteristics to behavior are the following: new psychological situations, overlapping psychological situations, egocentricity, and value loss. The authors show that when physical data are transformed to psychological data with the aid of these concepts, psychological understanding and the ability to make psychological predictions are possible.

Meyerson's review is in a similar vein. He discounts the value of much of the present empirical evidence on the grounds that it is relevant primarily for a sociology of children who attend special schools and classes. He suggests, first, that subjects attending special schools and classes are weighted for nonnormality in respects other than hearing; and, second, that the behavior of deaf children in residential schools is at least as much a function of their institutionalization as it is of their impaired hearing. A third neglected variable is that the same prenatal or postnatal illnesses that result in deafness may also cause other kinds of impairments. For example, if a child becomes both deaf and brain damaged as the result of an attack of meningitis, his mental retardation cannot logically be ascribed to his deafness. At the present time, there appears to be no study in which it can be said with assurance that the subjects had impaired hearing and no other uncontrolled disability or deprivation. There is an encouraging trend toward more adequate matching of experimental and control samples which may reveal whether deafness has any direct or indirect effects other than its influence upon the communication process. The present evidence indicates that deafness *per se*, like other physical variations, is not dynamically related to somatopsychological variables. It is psychologically neutral.

As Barker *et al.* (6) put it: (a) Physically deviant persons are not a homogeneous group psychologically. Physical disability is a phenotypic classification having about the same significance for the psychological investigator as red stones have for the geologist. (b) Lawful somatopsychological relations between physique and behavior are mediated by the psychological situation which physique helps to create in its role as a tool and as a social stimulus.

Some recent work of excellent design and fair quality can be interpreted as casting some doubt upon these conclusions. These researches have not received the attention they deserve partly because they are original efforts which are peripheral to the main lines of past investigations and they are difficult to assimilate. Myklebust & Brutten (66) studied the visual perception of 55 children in a residential school for the deaf. The subjects were

between 8 and 11 years old, within the normal range of intelligence and within normal limits in peripheral vision, visual acuity, fusion, and vertical and lateral balance. They were matched case for case on sex, age, intelligence, and years of institutionalization with a control group of normally hearing children resident in a home for orphans and dependent children. In addition, the deaf subjects were divided, according to rigorous, explicit criteria, into three etiological groups: endogenous, exogenous, and undetermined. The exogeneous group was heavily weighted with cases of presumptive central damage and it was both younger and of lower average intelligence than the endogenous group. Equated subgroups were established, however, in which there were no significant differences in IQ or C.A.

The results showed that the deaf subjects were significantly inferior to their normally hearing controls on the Marble Board, Figure-Ground, and Dot Pattern Tests. Moreover, for matched etiological groups, there were no statistically significant differences between the endogenous and exogeneous cases. On the average, the group of endogenous subjects tended to make better scores than the exogenous or undertermined groups, but they still performed less well on these visual perceptive tasks than their normally hearing controls. Part of these findings are in disagreement with Oléron's (68) conclusion that French deaf-mutes were not inferior in tachistoscopic perception.

In a less experimentally and logically rigorous paper, Myklebust (65) presented or referred to evidence which shows that samples of deaf subjects, undifferentiated with respect to other impairments, received lower scores on tests of motor capacity, peripheral vision, intellectual capacity, social maturity, and visual perception.

In the earlier study Myklebust & Brutton (66) concluded that deafness, *qua* deafness, disturbs or retards the visual perceptual processes. They postulated that a severe sensory impairment requires a realignment of organismic action patterns on both neural and psychological levels, particularly with respect to sensory interweaving. Their conclusions may be theoretically tenable. There is a firm base of experimental evidence, which they cite, that clearly demonstrates some inter-sensory relationships. The interpretation of these data with respect to the behavior of deaf individuals, however, is another matter. Myklebust & Brutton's findings may be accounted for in part by what appear to be two statistical errors. The first is the improper use of the "t" statistic for testing the significance of a difference between means when standard deviations are significantly different. The second is the failure to use either paired difference scores or the correlational term in the formula for testing the significance of a difference between correlated means. Nevertheless, enough possibly significant data remain which call for closer examination and explanation.

These authors' views are congruent with Werner & Wapner's (92) sensory tonic theory of perception and with Caplan's (19) and Feldenkrais's (33) phenotypically different but genotypically similar speculations about the

genesis of anxiety in motor-handicapped children. The line of investigation is surely worth following.

In his later paper, Myklebust (65) reached conclusions such as the following: "Deafness causes the individual to behave differently," and "deafness causes social immaturity." Here, however, he appears to be considering deaf individuals, who may frequently have other organic impairments and who in our culture may experience various degrees of social and psychological deprivations, as an homogeneous class. The author, quite properly in the reviewer's opinion, attempts to enlarge consideration of the problems of deafness from sole emphasis on simple deficit in communication to multivariate effects of impaired hearing on the total organism. It is by no means evident, however, that the empirical findings he describes are functions of deafness, *qua* deafness, and there are good reasons for believing that they are not (62).

Templin (81, 82), in a series of technically and statistically excellent studies, investigated the effect of environmental restrictions upon reasoning in children. Specifically, she hypothesized that environment may be restricted by an intrinsic factor of hearing impairment and by an extrinsic factor of residence in an institution. Her findings "largely supported" the hypothesis that reasoning ability varied inversely with degree and time of onset of impaired hearing, and "largely rejected" the hypothesis that residence in an institution was a significant variable. The conclusions were not clear-cut partly because of the confounding of physical, educational, and psychological variables. For example, the extrinsic restriction of the environment turned out to be essentially physically rather than psychologically restricting. Similarly, the methodology of the study required written answers for most of the questions, and it is known that the deaf are retarded in language.

Oleron (69) reviewed several studies of reasoning and conceptual thinking in which language was not required for a response. He calls attention to some ways in which the "concrete" reasoning behavior of deaf children is different from others who have difficulty in abstract thinking. Oleron, like Templin, ultimately reaches the conclusion that the deaf show retarded development rather than incapacity. This retardation is mediated by the psychological environment and is amenable to education and training.

On the social psychological frontier, Rutledge (76) studied aspiration levels. He used two motor tasks, one of which required good balancing skills. Inasmuch as deafness is often associated with damage to the semicircular canals of the ear, it was hypothesized that the deaf as a group would be less able than others on the balancing task. The results, while not entirely clear-cut, led him to conclude that institutionalized deaf children differ from institutionalized hearing children in level of aspiration only on tasks for which the deaf are intrinsically handicapped. The conclusion is tentative as it should be. This is an area in which further investigation seems likely to be rewarding for both somatopsychology and for general psychology.

Impaired hearing.—Except for psychophysical problems, investigations of impaired hearing of lesser degree than deafness are generally lacking. This lacuna may reflect the need for new theoretical positions which will point to important problems to be investigated. In the past, problems of impaired hearing were defined in terms of diseased ears and physiological impairments in ability to perceive pure tones. Somatopsychological results with this orientation, as might be expected, were inconclusive.

Meyerson (63) made an attempt to restructure the problem in terms of impaired ability to perceive speech. He conceptualized the process of perceiving, listening to, and comprehending verbal stimuli as "auding," and he standardized a test to measure this function. His experimental finding that impaired perception for speech may exist independently of impaired perception for pure tones has been supported by a factorial analytic study by Hanley (48). In practice, the view that auding is a skill that can be learned has been shown for the deaf (53), the hard of hearing (85), and the normally hearing (64).

Licklider (57) reviews other studies of the subjective attributes of sounds. Caffrey (18) offers a summary of the literature on auding. The latter is largely an educational literature, but it need not remain so. This is an area that is ripe for somatopsychological study. Relationships between impaired hearing and behavior that were obscure or inconsistent when the criterion of impairment was the pure tone audiogram may become clearer when the criterion is changed to a concept with psychological meaning such as auding.

CRIPPLING

The literature on crippling up to 1953 has been reviewed by Barker *et al.* (6), Cruickshank (23), and Hollinshead (52).

The quality of most of the studies in this field is not high. Some of the technical deficiencies noted in an earlier survey (6) are still present. These are the following:

(a) Omission of data on the life situation. Frequently all that can be assumed about a set of subjects is that they have some sort of variation in physique, but this cannot be taken for granted. Often the subjects are categorized only on a sociological variable such as place of residence. Transformation of the physical data into psychological data, even in simple terms such as the satisfyingness or the deprivingness of the physical situation, is rarely attempted.

(b) Inadequate data on behavior and personality. Paper and pencil personality inventories and projective techniques continued to be interpreted in terms of what the gross scores of configurations are supposed to mean for the nondisabled populations whose life situations are different. Item analyses are rare. Ratings are presented that appear to communicate more about the values of the raters than the characteristics of the ratees.

(c) Inadequate reporting. More than three-fourths of the serious studies examined in preparation for this review did not present sufficient data to

permit independent evaluation. This was especially true of articles published in clinical and medical journals. In view of known editorial pressures for shorter papers, however, it is not clear that responsibility for inadequate reporting falls solely on the investigator. A statement of subjects, methods, and results or judgments may be adequate for some fields in which standards of training and standardization of procedures are high and uniform; it is not adequate in somatopsychology.

(d) Conclusions based on improper statistical computations. A surprisingly common error in the period under review has been the use of a direct "t" test for the significance of a difference between means when the variances of the two groups were obviously significantly different. Failure to take advantage of the statistical benefits of pairing experimental and control subjects was also common.

Fortunately, positive gains are by no means absent. The frequency, if not the proportion, of high quality studies has been increasing. The use of more adequate control groups is common. Rarely does one find today an institutionalized disabled group compared with noninstitutionalized controls. Most important, it is evident that an increasing number of well-trained, methodologically and theoretically sophisticated investigators are at work.

Amputations.—The National Research Council of the National Academy of Sciences has supported a Committee on Artificial Limbs to the extent of one million dollars a year since 1945. Part of this support has been channeled to psychological studies. Abt (1) reviews what has been learned. Fishman (36) offers a psychologically sophisticated analysis of the problems and the process of evaluating the usefulness of an artificial limb.

Three experimental studies yielded divergent results. Noble *et al.* (67) reported that practically all his amputee subjects showed significant alterations in the Draw a Person Test, but Wille's amputees (93) did not. Wille concluded that in the Draw a Person Test amputees project their personality problems and not their body images. The amputee subjects of Noble *et al.* reported that the image of a man with an amputation was less distorted than the image of a normal man when both were viewed through aniseikonic lenses. This was interpreted as unconscious denial of the amputation. Wittreich & Radcliffe (94), however, showed that the same finding held for physically normal viewers. The significance of the latter results is obscure. Perhaps there is a generalized resistance toward perceiving mutilations that is dynamically similar to the resistance toward perceiving other kinds of "threatening" visual stimuli. It seems possible that an indirect measure of adjustment to amputation could be based upon this phenomenon.

Dembo & Tane-Baskin (27) reported a stimulating study of the social usefulness of the cosmetic glove. They showed that 80 per cent of the general public, to whom deliberate exposure was made, did not notice a difference between a subject's real hand and a cosmetic glove. Six artists for whom the amputees posed felt that the cosmetic hand did not fit the person but none recognized it as artificial. These findings are important to the negative feel-

ings of disabled persons to being exposed to pity, curiosity, and penetration of private regions. If public perception of mutilation is threatening to others, and if being perceived as mutilated is a source of disesteem to the amputee, one way of reducing stress seems clear.

Cerebral palsy.—Comprehensive book-length treatments of cerebral palsy have recently appeared (20, 21). It is now well established that at least 45 per cent of cerebral palsied children test at the mentally defective level when measured with the Stanford Binet. In addition, they may frequently have other physical defects and perceptive disorders. Cruickshank (23) has identified at least eight combinations of these variables. In such a multiply handicapped group it is difficult to isolate the somatopsychological problem. Some progress, however, has been made.

Wenar (87) found that on a motor task cerebral palsied children differed from normal children in pattern of level of aspiration behavior. This finding is similar to previous studies (76) which have shown that deviate aspiration behavior results when the task is one on which the child is intrinsically handicapped.

World Test results, according to Wenar (88, 89), showed decreased integrative and defensive ability for the motor handicapped children with greater handicap leading to greater impairment. However, there were no significant differences between normal and handicapped groups in over-all adjustment, variety of interest, concern over interpersonal relations, or number of destructive fantasies. Wenar emphasized the large overlap between the groups. He called particular attention to the restitutive and compensatory mechanisms that are available to handicapped children but which may be underestimated or neglected. He did not find a necessary relationship between motor handicap and emotional disturbance. Wenar's results are similar, in many respects, to those obtained by Barker & H. Wright (7), although the latter used different kinds of handicapped children and different methods.

An excellent study, with important practical implications, by Garmezy & Harris (42) reinforces the conclusion that basic psychological processes in physically handicapped children are not different from others as a function of the disability. They found that the effects of motivation on learning in cerebral palsied children were similar to the effects reported for non-handicapped children. Sheer (79), on theoretical grounds, reached the same conclusion.

The practical meaning of the Garmezy & Harris (42) pioneering study should be emphasized. It points clearly to the relative ineffectiveness of the verbal praise that is constantly used by auxiliary medical therapists, and it offers better methods. Aside from a methodologically inadequate study of the effects on personality of occupational therapy (12), this research seems to be the only report in which psychologists have tried to make a contribution to the important motivational problems of physical rehabilitation. Expansion of effort in this area would be desirable and helpful.

Several minor reports on cerebral palsy may be mentioned. The clinical opinions of a medical authority (70) concerning a direct relationship between kind of cerebral palsy and kind of behavior received no support from two experimental investigations (15, 42). Greenbaum *et al.* (46) found that presenting cerebral palsied children with a TAT-like test showing crippled characters was not more valuable in revealing personality than the regular TAT (Thematic Apperception Test). Many articles state that the cerebral palsied are emotionally maladjusted but none examined reached acceptable levels of scientific reporting. Denhoff & Holden (28) and Bice (13) are in agreement that parental attitudes significantly influence adjustment, although they do not show how it is mediated. Wortis (95) and Gallagher (41) illustrate the increasing recognition that attitudes of parents are not necessarily "rejecting" but reflect the difficulties of solving reality problems that are associated with cerebral palsy in our culture. A study by Shere (80) of twin pairs, one of whom was cerebral palsied, supports theoretical expectations that this method is potentially fruitful.

Poliomyelitis.—The psychological aspects of poliomyelitis has never attracted many investigators in comparison with the amount of effort devoted to other aspects. Only a handful of studies have appeared in the last four years, and in view of the discovery of practical medical methods for the control of the disease, it seems unlikely that research output will be increased in the future.

Seidenfeld (77) outlined an excellent program for the investigation of the psychological consequences of breathing difficulties. Wendland (90, 91) studied the employment prognosis and the religious feelings of the postpoliomyelitic. The data indicated that the work statistics of postpoliomyelitics who have no residual paralysis or only slight involvement compare favorably with a comparable nonhandicapped population. In the religion study, the absence of a control group prevents comparison.

SUMMARY

The present status of somatopsychology may be considered simultaneously gratifying and discouraging. The reasons for gratification are many. The increase in research effort within the last five years has been striking. Progress in clarifying difficult theoretical and conceptual problems and in communicating possible solutions has been evident. Methodological difficulties with respect to sampling, to descriptive procedures, and to experimental and statistical problems have been increasingly recognized and some degree of control is more frequently imposed. New and improved tools for investigation have been developed and are in use. Some practical problems have been solved; encouraging gains in understanding others have been achieved, and new areas for productive work are beginning to be seen. Perhaps most important, a small but active group of investigators is at work.

The discouraging aspects reflect primarily the immensity of the need in comparison to the talent and resources available. The high ratio of popular

articles to scientific studies in this field indicates the extent of the demand for knowledge. It is not surprising that this demand should attract those with good intentions as well as those with adequate skills. Stimulus-bound, tool-bound, "one-shot," data-fishing expeditions with inadequate attention to the need for obtaining meaningful data are more or less inevitable outcomes. The relative ease of obtaining financial support for service projects as compared to the difficulty of obtaining support for research is the second aspect of this problem. Needed studies of well-adjusted disabled persons and their situations and longitudinal studies, for example, are infrequently sponsored.

The immediate outlook for further progress, however, is not unhelpful. Among the most promising developments is the recent initiation of a program by the United States Office of Vocational Rehabilitation for the training of rehabilitation counselors and the support of research. This program may be as fruitful for the development of somatopsychology as the support of the Veterans Administration has been for clinical psychology.

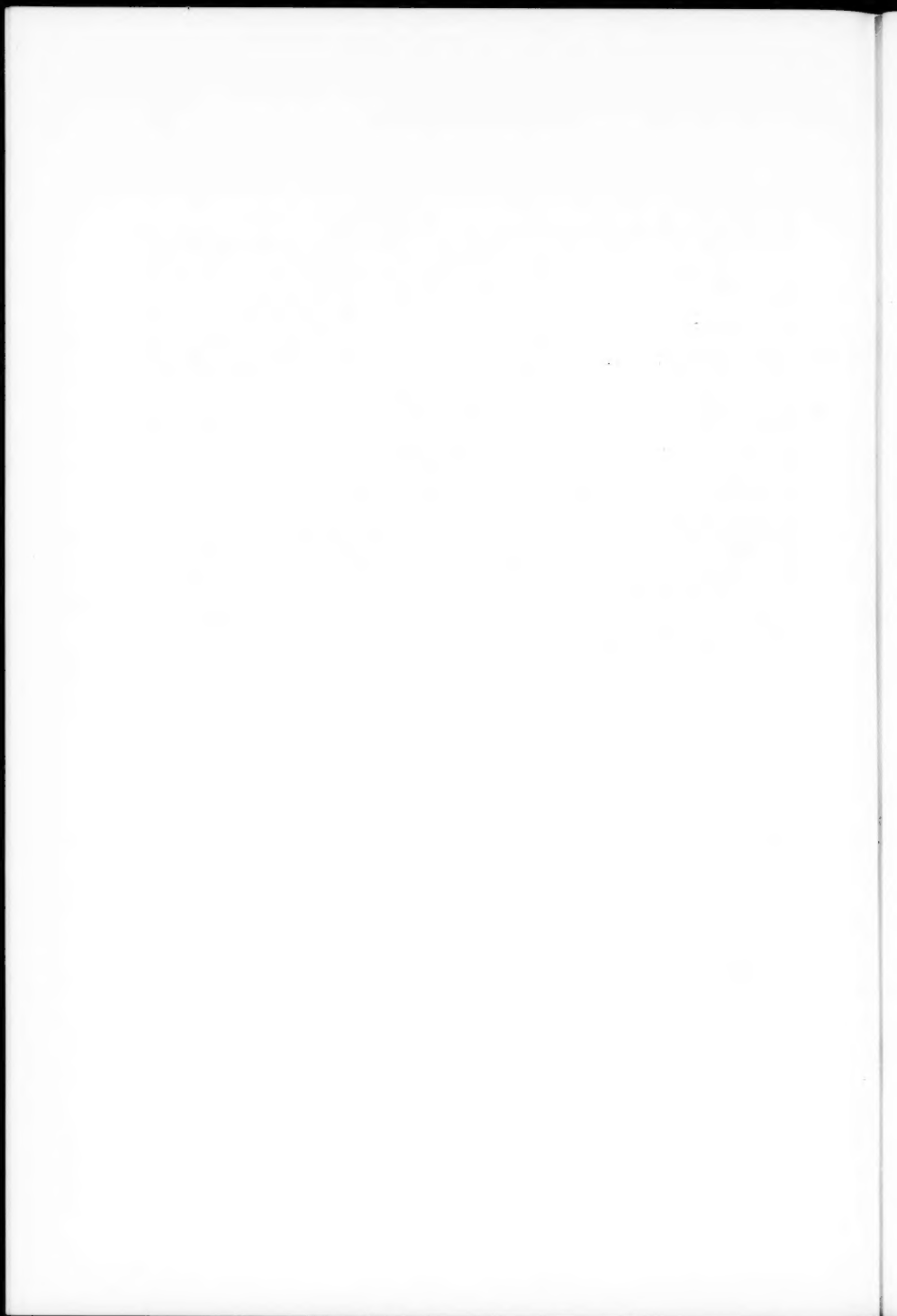
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